

# THREE ESSAYS ON LABOR DEMAND AND SUPPLY

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by

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# THREE ESSAYS ON LABOR DEMAND AND SUPPLY

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*Abstract:* The following essays are concerned with the issues of labor supply and demand. Each essay's topic addresses the economic analysis of a wage floor or, in the case of the first essay, the Earned Income Tax Credit, which is often analyzed as an alternative policy to a minimum wage.

Chapter 1 examines the response of workers, in terms of hours worked, to the Earned Income Tax Credit. Several studies have found that receipt of the EITC induces single women with dependent children to enter the labor market. Employing a regression kink design, I exploit the discontinuities in the EITC benefit function to estimate the impact of benefit receipt on single mothers hours of work once in the labor market. I find a decrease in hours worked for mothers with more than one child. The estimate is statistically significant but economically small. Chapter 2 investigates living wage laws, estimating wage, establishment number, and total employment for industries likely covered by a living wage law. I find that, contrary to expectation, living wage laws increase wages for covered industries but do not lead to firm exit or relocation or increased unemployment. Chapter 3 also looks at living wage laws. I use propensity score matching to match individuals in the two types of cities in an attempt to overcome this weakness in identification. Like previous studies, I find a statistically significant, positive effect of living wage policies on wages for those in the lowest decile of the wage distribution. However, I find an effect on employment and hours worked that is not different from 0.

## BIOGRAPHICAL SKETCH

Margaret (Maggie) R. Jones earned her B.A. in Classics from the University of Massachusetts, Amherst, in 1991. After a 10-year career as an editor, writer, and production manager in the magazine- and book-publishing industry, she enrolled in the Muskie School of Public Service in 2001, earning an M.A. in Public Policy and Management in 2006. While at Muskie, she focused on policy analysis and empirical research methods, and wrote a thesis on the food security status of H2A apple harvesters in Maine. She also performed research and wrote reports for a variety of agencies in Maine on the topics of poverty, nutrition, and smart-growth planning. She then enrolled in the Department of Policy Analysis and Management at Cornell University, earning a PhD in Policy Analysis with a concentration in Economics. At Cornell, she focused her research on labor economics, wages, and labor supply and demand, and plans to continue this research as an economist with the U.S. Census Bureau.

To Karl.

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# CHAPTER 1

## THE EITC AND LABOR SUPPLY: EVIDENCE FROM A REGRESSION KINK DESIGN

### 1.1 Introduction

This paper estimates the effect of the federal Earned Income Tax Credit (EITC), the largest cash-transfer program in the United States, on the labor supply of single mothers at the intensive margin. Economic theory predicts that the provision of an EITC should induce labor force participation (the extensive margin) of single mothers, and previous research has documented this relationship (Eissa and Hoynes, 2006; Meyer and Rosenbaum, 2001). The EITC is theorized to have an ambiguous effect on hours worked depending on where a worker's income places him or her along the benefit function. When the credit is increasing as a function of earned income, hours may increase or decrease; and at points where the credit plateaus or decreases, hours are predicted to decrease.

So far, studies have not found convincing evidence in support of the theory described above. Previous work has focused on discerning the intensive response by using increases in EITC generosity as a source of identification (Eissa and Liebman, 1996; Eissa and Hoynes, 2006), or by exploiting the variation in state tax structure, including state-specific EITCs (Meyer and Rosenbaum, 2001). Many studies rely on a difference-in-differences approach, using single women without children as a "control" for single mothers, or single mothers with one child as a "control" for those with more than one.

Eissa and Hoynes sum up the apparent lack of hours response by stating: "A consistent and somewhat puzzling finding in the empirical literature on the EITC and labor supply is the large response of the participation decision and the lack of any response in the reported hours worked by taxpayers in the labor force" (Eissa and Hoynes, 2006, p.

102). They offer several explanations: the hours worked elasticity is simply too small to discern; workers are unable to choose their hours but rather rely on institutional norms or employer expectations; or, workers are unaware of the structure of the EITC.

In the work presented in this paper, I rely on the discontinuity in the EITC benefit function to discern an hours worked response. The research design is very well suited to the question, and constitutes an improvement over previous research since it permits comparison of female workers within the same group (those with one child or more than one child), rather than relying on a comparison between groups. The analysis also relies on workers' updating their information and on the time-order of questions on income, earnings, and hours worked asked in the March Supplement to the Current Population Survey. Chetty and Saez (2009) discuss the assumption made in the literature on tax and transfer policy that individuals are fully informed about the structure of these policies, and that they use this information in making choices. They test this knowledge by running an experiment on H&R Block clients. Tax professionals who provided their clients with information on the structure of the EITC induced their clients to increase their earnings, and thus their EITC refund, in the next period. Similarly, the analysis presented here relies on workers updating their understanding of the EITC upon doing their taxes.

The mechanism being investigated is thus different from standard labor supply analyses that attempt to estimate substitution and income effects. These analyses presume foreknowledge of credit receipt on the part of workers that permits them to make a simultaneous earnings and hours decision. Yet the overwhelming majority of taxpayers have imprecise information about the EITC, and they receive the credit as a lump sum in one point in time. In other words, the likelihood is small that workers know enough about the credit to treat it as a wage subsidy and to adjust their hours simultaneously with their earning the credit. On the other hand, by looking at hours worked around the time that these same workers are filing their taxes (in other words, when their own

EITC benefit becomes known to them), an hours-worked response to the EITC may be discerned.

The results of this analysis indicate that single mothers with more than one child reduce the number of hours that they work when their income in the preceding period places them immediately after the kink in the EITC benefit function where the benefit begins to decrease. I also find a similar effect for mothers with one child, but this effect is not as robust. I find no effect at the kink where earners enter the plateau region. This is likely due to other elements of tax and transfer policy that cloud the perception of loss at this point, or it may indicate that receipt of the EITC does not induce an income-effect response.

The paper is organized as follows: In section 2, I present background on the EITC and previous research pertaining to its effect on the labor supply of single mothers. Section 3 covers the regression kink design, its applicability to my research question, and the identification strategy and assumptions used in the analysis. Section 4 describes the data, including a discussion of density and covariate tests. The main results are presented in section 5, and robustness checks pertaining to those results in section 6. Section 7 suggests some policy implications and concludes the paper.

## **1.2 EITC Background and Previous Research**

### **1.2.1 Background**

The EITC was instituted in 1975 and underwent major changes over the years, particularly in 1993 in coordination with welfare reform. It is a refundable credit, meaning that it provides a credit to taxpayers even if they have no federal tax liability. Taxpayers are eligible for the credit if they earned a wage and salary income over the last year and are under

a certain maximum of total income. A negligible credit is available to workers without children; workers with qualifying children (who meet certain residency restrictions) are eligible for credits that in some cases represent a large fraction of income.

Figure 1.1 shows the structure of the EITC as a function of earnings for single tax filers with eligible dependents.<sup>1</sup> The upper line in the figure shows the benefit structure for those with two or more children, and the lower line the benefit for those with one child. Eligible dependents include children younger than 19, or younger than 24 and a full-time student. For each category of family, the shape of the benefit function is the same, but the level of benefit is substantially higher for those with two or more dependents compared to those with one.

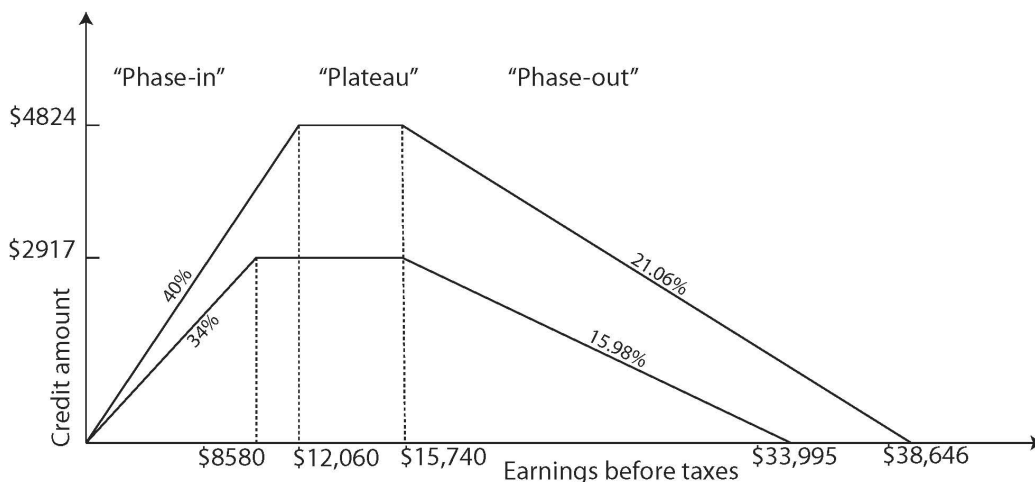


Figure 1.1: EITC parameters, 2008

As in the figure, in 2008, upon earning income greater than 0, a working mother with one child receives a credit of 34% of income up to a maximum credit of \$2917, which occurs at earnings of \$8580 (the phase-in region). She then enters the plateau region, over which she receives \$2917 in EITC regardless of how much more she earns. At an earned

<sup>1</sup>Figure 1.1 is adapted from a similar figure in Meyer (2002).

income of \$15,740, she then faces a decreasing credit equal to the maximum credit minus 15.98% of income, until the credit is completely phased out at earnings of \$33,995. For women with two or more children, the maximum credit is \$4824 (at \$12,060 in income), the phaseout income is \$15,740 and the phase-out rate is 21%, with final eligible earnings of \$38,646.<sup>2</sup>

The EITC exhibited the structure shown in Figure 1.1 from 1996 to 2008. Beginning in 2009, a higher level of EITC generosity was legislated for women with three or more children.

Since its introduction, many states have also adopted EITCs, most of which are a percentage of the federal EITC. The majority of these are quite small, representing between 3% and 5% of the federal credit.

The EITC is hypothesized to induce work among single mothers not currently in the workforce. For those already in the workforce, the effect of the credit on hours worked depends on which region of the credit a worker's earnings puts her.

Figure 1.2 shows simplified versions of the EITC with indifference curves drawn to demonstrate the hypothesized effect of the credit on labor supply.<sup>3</sup> Each panel shows a diagonal line that represents the worker's original budget constraint, with leisure a normal good with a value of 0 hours at the origin. The addition of the EITC benefit yields a new budget constraint. First, it is clear from each panel that the benefit provides at least as much income as before for every choice of hours. The credit increases the hourly wage of each worker. In standard labor-supply analysis, a worker's hourly wage is the hourly price of her "leisure."<sup>4</sup> When her wage rate increases, she substitutes away from leisure

---

<sup>2</sup>Liebman (1998) discusses the history of the EITC at length. Its shape is largely due to expansions and compromise legislation that appear to be uncoordinated with other tax and transfer programs. The plateau region, for example, resulted from a compromise expansion in the program that occurred in 1979.

<sup>3</sup>Figure 1.2 is adapted and simplified from similar figures in Hoffman and Seldman (2003).

<sup>4</sup>I put "leisure" in quotation marks because it is a catchall phrase for activities other than paid work. Taking care of children and households are more likely to be the activities undertaken by this sample of women.

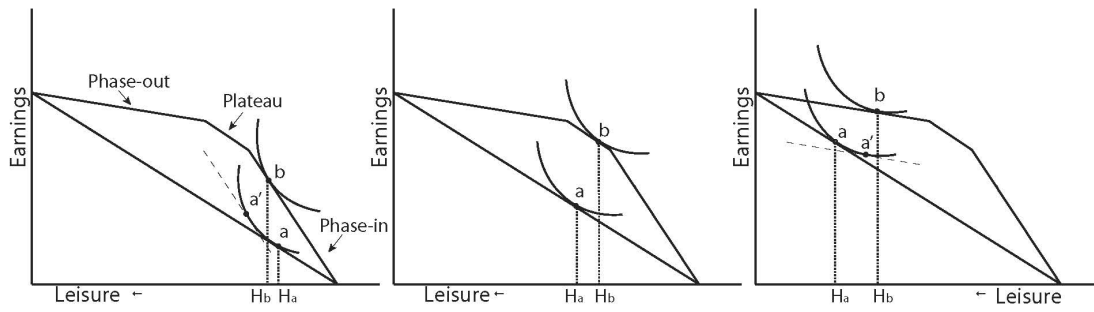


Figure 1.2: EITC substitution and income effects (see text for description)

since leisure is now relatively more expensive (the “substitution effect”). However, if leisure is a normal good, an increase in income results in more consumption of leisure (the “income effect”).

Each panel shows the interaction of income and substitution effects and their theoretical impact on hours worked. The panel on the left shows the case for the area of increasing benefit. In the absence of the EITC, the worker chooses  $a$ , with hours of work  $H_a$ . The substitution effect appears as a movement from  $a$  to  $a'$  along the income-compensated (dotted) budget line. The income effect—the vertical distance between the income-compensated line and the new budget constraint—causes hours to decrease to  $b$ . Note that, depending on the sizes of the income and substitution effects, hours could decrease or increase in this case.

The middle panel shows the case when the worker’s earnings place her in the plateau region. Here, the two budget lines are parallel, and only an income effect exists (since the credit is constant over this region regardless of further hours worked). Hours are predicted to decrease, from  $H_a$  to  $H_b$ .

Finally, the last panel shows the case when earnings place the worker in the region of decreasing benefit. The worker still receives more income from the credit, but for ev-



ery further hour worked, her wage decreases, making leisure less expensive. Hours are predicted unambiguously to decrease.

### **1.2.2 Previous Research**

Empirical studies of the EITC have consistently found that the credit increases labor force participation of single mothers at the extensive margin (meaning any participation in the labor force). Eissa and Liebman (1996) used the expansion in the EITC in 1986 to estimate the labor supply response of single women with children. They employed a difference-in-differences approach to compare women without children and those with children before and after the policy change. They found that labor force participation rates went up for women with children by about 2.8 percentage points compared with women without children, with higher participation rates for women with less than a high school education. They found no evidence that the EITC decreased hours of work for women already in the labor force; rather, they found a positive effect on hours worked that was not statistically significant.

Meyer and Rosenbaum (2001) analyzed the EITC in the larger context of welfare reform, comparing the effects of the EITC in tandem with welfare and Medicaid receipt and state and federal tax changes. They found that the 1996 expansion of the EITC and other tax changes accounted for the larger share of increase in labor participation by single mothers, with welfare changes accounting for a smaller share. Specifically, they find that the weekly employment of single mothers with one child relative to women without a child rose 7.1 percentage points between 1984 and 1996, a difference they attribute largely to the EITC.

In a study of California welfare recipients, Hotz and Scholz (2006) found a similar employment effect of an expansion of the EITC. As in Eissa and Liebman (1996), the authors

use the greater generosity of the EITC for those with more than one child to distinguish the effect of the credit.

In the foregoing studies, no convincing evidence of an intensive margin effect was found for this population; one explanation for this is that the single mothers in question might not have been aware of the EITC's structure, and thus could not take EITC receipt into account as though it were a wage subsidy. A requirement for a simultaneous earnings/hours decision in the presence of a wage subsidy it that the subsidy be known.

In a study using H&R Block offices, Chetty and Saez (2009) found that clients who were given information about the structure of the EITC when their taxes were being prepared increased their EITC in the next tax year. The effect, however, occurred among filers who were clients of so-called "compliers"—tax preparers who informed clients how to maximize their EITC. Clients of "non-complying" tax preparers maximized earnings, even when those earnings placed them in the phase-out region of the EITC schedule. Chetty and Saez (2009) conclude from their study that the intensive-margin labor supply response to the EITC is attenuated by a lack of information.

Similar to Chetty and Saez (2009), this analysis relies on the belief that workers change their behavior due to new information: namely, that those who find that their earnings have put them immediately beyond the plateau area of the EITC in one period might respond by decreasing hours of work in the next period. Unlike Chetty and Saez (2009), I am not able to observe the kind of information that workers receive when they file their taxes. However, previous research on how much people know about the EITC supports the assumption that at least some workers will become informed about it through tax filing (Maag, 2005). Thus, this paper contributes to the discussion by merging two strands of inquiry into the EITC, using new information on updating to help shed light on an older question regarding intensive labor supply.

## 1.3 Research design

### 1.3.1 Method

The regression kink design is a relatively new method and little exists in the way of its application. Many of the theoretical issues applicable to regression discontinuity designs also apply to this method. Because it relies on kinks (rather than jumps) in an otherwise-continuous policy variable that assigns a treatment, the EITC is particularly well-suited to this type of analysis. As in RD designs, kinks in the policy assign observations to one treatment or another in a manner that is (hopefully) as good as random. Card et al. (2009) formalized the design, relating it to regression discontinuity and providing formal proofs for the circumstances under which it can be used. The authors also connected the two strategies in terms of necessary tests and robustness checks, which are themselves outlined by Lee and Lemieux (2010). I adopt these strategies in this analysis.

The regression kink design relies on a regressor (such as a policy variable) that is a deterministic function of a “behaviorially endogenous variable” that cannot be replaced by a plausible instrument (because the outcome of interest is also associated behaviorally) (Card et al., 2009). The EITC benefit is determined by a worker’s earned income, and thus earnings and EITC receipt are together related behaviorally to hours of work. Thus no instrument exists for EITC receipt that can satisfy the independence assumption. However, because there are kinks in the benefit function, these can be exploited.

The process is related to RD designs in that a function, earnings, determines the EITC benefit received, and it is continuous everywhere except at two points. For a researcher or policymaker, these discontinuities are known; for workers, precise foreknowledge of where the kinks occur is unlikely and can be tested. Earners are therefore assigned to either side of the kink “as good as randomly,” and earners immediately before and after

the kink should look the same on all covariates. Unlike RD designs, the EITC function does not determine whether or not a treatment is given. Rather, the slope of the treatment function changes at a kink point.

To put it more formally: assume that  $Y$  is the number of hours worked per year in year  $t + 1$ ,  $V$  is earnings in year  $t$ , and  $B$  is the EITC, which is a deterministic function of  $V$  with a kink at  $v = 0$ . A simple formulation would be

$$Y = \tau B + g(V) + \epsilon, \quad (1.1)$$

where  $g(V)$  is a continuous function expressing earnings.  $B = b(V)$ , and is a deterministic function of  $V$  continuous everywhere except for a kink at  $v = 0$ . Then  $\tau$  can be estimated using the “RKD estimand”:

$$\tau = \frac{\lim_{v \rightarrow 0^+} \frac{\partial E[Y|V = v]}{\partial v} - \lim_{v \rightarrow 0^-} \frac{\partial E[Y|V = v]}{\partial v}}{\lim_{v \rightarrow 0^+} \frac{\partial b(v)}{\partial v} - \lim_{v \rightarrow 0^-} \frac{\partial b(v)}{\partial v}} \quad (1.2)$$

In words, the coefficient of interest,  $\tau$ , is the change in slope in the conditional expectation of  $Y$  (hours worked) at the kink point, divided by the change in slope of the benefit function (the change in the EITC at the kink point). Econometrically, the numerator can be estimated using the following model:

$$E[Y|V = v] = \alpha_0 + \sum_{p=1}^p [\alpha_p(v - k)^p + \beta_p(v - k)^p * D], \quad (1.3)$$

where  $|v - k| \leq h$ , and  $h$  is the bandwidth chosen. In this case,  $k$  is the kink point in question. The  $\alpha$ 's and  $\beta$ 's are the coefficients on the polynomial terms (and, after considering higher polynomial orders,  $p$  ranges from 1 to 3). The numerator of the RKD estimand is the coefficient  $\beta_1$ . The denominator is the derivative of the benefit function

at the kink, which is a simple mathematical calculation. The estimand can be considered the “treatment on the treated” (TT) effect as from a randomized experiment, as long as certain conditions are met regarding the distribution of all other pre-determined factors (Card et al. (2009)).

The effect I am able to estimate is the intent-to-treat. Information on actual EITC receipt is not available in the CPS (although the value of the benefit is imputed for eligible observations). My estimate, therefore, necessarily underestimates the full treatment on the treated effect. (According to Scholz (1994), 80% to 86% of those eligible each year for the EITC receive it.)

Some other examples of RKD’s include Guryan (2001), who used kinks in state education aid formulas as instruments to look at the effect of public school spending. Nielsen et al. (2010) used a kinked aid scheme to study the impact of direct costs on college enrollment.

### **1.3.2 Identification**

For this research design to be feasible, some assumptions about workers’ and tax filers’ behavior must be met. First, workers may not know the exact benefit structure of the EITC until they work on their taxes. Over the tax year in question, precise knowledge of kink points may allow workers to change their behavior, leading to bunching at kink points. For workers who are not self-employed, this foreknowledge is unlikely, since changes in the kink points occur each year as the benefit is adjusted using the Consumer Price Index. Even if they received the EITC in an earlier year, as long as workers cannot precisely assign themselves to a preferred position on the benefit function, this assumption is not violated (Lee and Lemieux, 2010).

Second, filers must gain knowledge of where their preceding year’s earnings have

placed them on the benefit function, and they must learn this information by March of the next year, when the survey is administered. This requires that some proportion of taxpayers does their taxes by March. Scholz (1994) found that more than half of all federal tax returns are filed between January and March, and expectation of a refund is associated with earlier filing. This provides some evidence that a respectable portion of the sample either files in time for the March survey, or at least have their tax calculations underway.

Finally, filers must become aware of the fact that a given level of earnings has placed them along a portion of the EITC benefit function that they consider undesirable, causing them to update their information and change their behavior. It is not necessary that filers learn the specific shape of the EITC in order to receive information that may cause them to update: Figure 1.3 shows the EITC schedule as taxpayers see it. A single mother with two children who earns \$15,750 in 2008 may look at the schedule and immediately see that she is just beyond receipt of the maximum benefit. In this information updating, the role of tax preparers is uncertain. Maag (2005) provides evidence that low-income earners report “hearing of” the EITC more often if they do their own taxes, but the difference between this group and those who use tax preparers is small (72% of those doing their own taxes have heard about the EITC compared with 69% of those who use tax preparers).

As in a regression discontinuity design, all other covariates must be smooth in relation to the kink points in the EITC. Of particular importance for this situation are other tax and transfer programs that may change in close proximity to the EITC’s kinks or may themselves be governed by rules similar to those that govern the level of EITC receipt.

| Earnings range |        | Dependents |       |           |
|----------------|--------|------------|-------|-----------|
|                |        | 0          | 1     | 2 or more |
| 15,250         | 15,300 | 0          | 2,917 | 4,824     |
| 15,300         | 15,350 | 0          | 2,917 | 4,824     |
| 15,350         | 15,400 | 0          | 2,917 | 4,824     |
| 15,400         | 15,450 | 0          | 2,917 | 4,824     |
| 15,450         | 15,500 | 0          | 2,917 | 4,824     |
| 15,500         | 15,550 | 0          | 2,917 | 4,824     |
| 15,550         | 15,600 | 0          | 2,917 | 4,824     |
| 15,600         | 15,650 | 0          | 2,917 | 4,824     |
| 15,650         | 15,700 | 0          | 2,917 | 4,824     |
| 15,700         | 15,750 | 0          | 2,917 | 4,824     |
| 15,750         | 15,800 | 0          | 2,912 | 4,817     |
| 15,800         | 15,850 | 0          | 2,904 | 4,806     |
| 15,850         | 15,900 | 0          | 2,896 | 4,796     |
| 15,900         | 15,950 | 0          | 2,888 | 4,785     |
| 15,950         | 16,000 | 0          | 2,880 | 4,775     |

Figure 1.3: Schedule of EITC used by taxpayers

## 1.4 Data

### 1.4.1 Data sources, sample, and measures

The data used in this paper are the March Supplements to the Current Population Survey, a nationally representative study of about 60,000 households. In the survey, participants are asked detailed questions regarding hours worked in the last week at all jobs, as well as information on earnings and weeks worked in the year immediately preceding the survey. Survey years included are 1997 to 2009. Due to the question structure of the CPS, using these years means including income, earnings, and tax information from 1996 to 2008, a time period over which EITC generosity remained the same in real terms for single parents. For every year, I create a variable that reflects earned income in 2008 dollars, and determine at what value of real earned income a kink in the benefit function occurs.

The dependent variable of interest is number of hours worked at all jobs per week, multiplied by the number of weeks worked in the preceding year (for ease of interpreta-

tion).<sup>5</sup> The questions asked regarding earnings and hours are crucial to the story being told in this analysis. Respondents are asked how many hours per week they usually work at all jobs at the time of the survey. In other words, for a given year of the March CPS, hours of work are applicable to March of that year. Earnings, income, and all other tax-related variables are asked for the year immediately preceding.<sup>6</sup>

I limit the analysis to single women between the ages of 16 and 55 who were eligible for the Earned Income Credit. Women were included if they had one or more children, had positive hours of work at the time of the survey, and reported positive earned income for the preceding year. Women were excluded if they reported zero hours of work or zero income, or if they reported being a full-time student. Women were also excluded if their investment income in the preceding year made them ineligible for the EITC. Finally, self-employed women were excluded, since there is an inducement for them to manipulate their earnings to where the EITC is maximized (and there is evidence that they do so (Saez, 2010)).

The benefit parameters of the EITC vary based on number of children, with women with one child receiving less per dollar earned over the entire benefit function compared with women with more than one child.<sup>7</sup> Because the treatments are different, I split the sample into two groups: women with one eligible child and women with more than one. There is precedent for treating the groups separately due to the greater generosity of the credit for those with more than one child. Meyer and Rosenbaum (2001), for example, use single mothers with one child as a control group for single mothers with two or more. As a robustness check, I also run the analysis on women without children. The number

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<sup>5</sup>Single mothers receiving the EITC may also respond in the number of weeks worked. To try to overcome this potential endogeneity, I used the mean and the median for all single mothers as the multiplier. The results were qualitatively similar.

<sup>6</sup>The following question was asked in March 1997 regarding earnings: "How much did (name/you) earn from this employer before taxes and other deductions during 1996?"

<sup>7</sup>In 2009, the benefit structure was changed to give a larger benefit to single parents with three or more children, creating three levels of benefits for single parents with any children.



of eligible children was calculated from the dependency status indicator in the CPS.<sup>8</sup> Dependents who meet certain age and status requirements were counted as eligible children for both the EITC and other tax variables used as covariates.

The analysis is performed for the two groups within different bandwidths of the assignment variable, earnings, around two kinks in the benefit function: the “first” kink, where earners leave the area of increasing benefit and enter the plateau; and the “second” kink, where earners leave the plateau and enter the area of decreasing benefit.<sup>9</sup> The bandwidth choices for each group are also constrained by the rules of the benefit function, since a bandwidth that includes observations too close to the non-relevant kink might induce bias. Bandwidths of \$1000 and \$2000, in 2008 dollars, were chosen for both groups. Then, since the plateau region is “wider” for women with one child compared to those with more than one, a bandwidth of \$3000 was also used for this group.<sup>10</sup>

The regression kink design, as with regression discontinuity, relies on the smoothness of covariates in relation to the forcing variable. Likely the most important covariates to

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<sup>8</sup>Each observation receives a marker indicating the person in the family upon whom he or she is dependent, or is coded 0 if not dependent on another.

<sup>9</sup>I could also have considered the kink where earners have left the benefit entirely to see if their hours decrease in the next period. The marginal benefit from making this choice at this level of earnings, however, is vanishingly small. Earners would have to reduce their hours substantially before gaining back in EITC what they would lose in earnings from the foregone hours.

<sup>10</sup>Bandwidth choice was tested using a variety of tests suggested in Lee and Lemieux (2010). Bandwidth was originally chosen largely because of the constraints in the benefit formula, which leave ranges in the earnings function where a “kink” could be considered applicable. Once chosen, bandwidths were weighed against one another using a “leave one out” approach described by Lee and Lemieux (2010), via Imbens and Kalyanaramang (2009), which chooses the bandwidth with the lowest squared error between predicted and true values of the outcome variable. Further, a more objective “rule of thumb” test for a rectangular kernel was run for women with more than one child at the second kink:

$$h_{ROT} = 2.702 \cdot \left( \tilde{\sigma}^2 R / \sum_{i=1}^N [\tilde{m}''(x_i)]^2 \right)^{1/5}$$

where  $m''$  is the second derivative of an estimated regression of hours on earnings,  $\sigma$  is the estimated standard error of the regression, and  $R$  is the range of earnings. A relevant range of earnings was selected for this group, beginning at the first kink and ending at the point where the EITC credit goes to 0. The test calculated \$970 as the optimal bandwidth.

check in this case involve the entire tax burden and transfer benefit that a mother in the sample faces. Because the EITC is only part of a package of burdens and credits, and because tax burden will vary by year and state, two concerns are paramount: that the overall tax burden doesn't change abruptly at the same level of income as does the EITC; and that no other credit or benefit uses the same income cutoffs to determine eligibility. Tax information was generated using NBER's TAXSIM program (Feenberg and Coutts, 1992), and this information was merged with the CPS data.

Other covariates in the CPS include attributes that likely influence both hours of work and earnings, such as age, age of youngest child, race, ethnicity, education, and overall number of children for the group with more than one. Table 1.1 provides summary statistics for the covariates considered in the analysis for each group and bandwidth.

### **1.4.2 Density and covariate tests**

Best practices for regression discontinuity designs, outlined by McCrary (2008) and summarized by Lee and Lemieux (2010), require an examination of the density of the assignment variable to ensure that no one is "gaming" the system, but that assignment to one side of a cutoff or the other is as good as random. The most straightforward way to check that this requirement is met is an examination of the histogram of earnings to see if observations are bunched up immediately before a kink point. Figure 1.4 and Figure 1.5 show histograms of earnings for the two groups of single mothers within \$2000 (scaled to real 2008 dollars) on either side of each kink point. There is clear evidence of heaping (rounding) in the assignment variable at certain values, but no evidence of bunching before a kink that occurs independent of this heaping. (Since attributes of workers who round earnings may influence the dependent variable, leading to biased estimates, I check for this by employing a test suggested by Barreca et al. (2010), described later.)

Table 1.1: Summary statistics

| One child             | First kink |         |         |         |         |         | Second kink |         |         |        |         |        |
|-----------------------|------------|---------|---------|---------|---------|---------|-------------|---------|---------|--------|---------|--------|
| Bandwidth<br>Variable | \$1,000    |         | \$2,000 |         | \$3,000 |         | \$1,000     |         | \$2,000 |        | \$3,000 |        |
| White                 | Mean       | SD      | Mean    | SD      | Mean    | SD      | Mean        | SD      | Mean    | SD     | Mean    | SD     |
| Black                 | 0.73       | 0.44    | 0.73    | 0.44    | 0.72    | 0.44    | 0.71        | 0.45    | 0.71    | 0.45   | 0.72    | 0.45   |
| Other                 | 0.21       | 0.41    | 0.21    | 0.40    | 0.22    | 0.41    | 0.22        | 0.42    | 0.23    | 0.42   | 0.22    | 0.42   |
| Age                   | 0.06       | 0.23    | 0.06    | 0.24    | 0.06    | 0.24    | 0.06        | 0.25    | 0.06    | 0.24   | 0.06    | 0.23   |
| Less than high school | 33.04      | 10.06   | 33.16   | 10.02   | 33.23   | 9.96    | 35.02       | 9.76    | 34.94   | 9.79   | 34.99   | 9.79   |
| H.S. degree           | 0.19       | 0.39    | 0.18    | 0.39    | 0.19    | 0.39    | 0.18        | 0.39    | 0.19    | 0.39   | 0.19    | 0.39   |
| Some college          | 0.44       | 0.50    | 0.43    | 0.49    | 0.43    | 0.49    | 0.43        | 0.50    | 0.43    | 0.50   | 0.43    | 0.50   |
| College degree        | 0.30       | 0.46    | 0.31    | 0.46    | 0.30    | 0.46    | 0.32        | 0.47    | 0.32    | 0.46   | 0.32    | 0.47   |
| Post grad             | 0.06       | 0.23    | 0.06    | 0.24    | 0.06    | 0.25    | 0.05        | 0.22    | 0.05    | 0.23   | 0.05    | 0.22   |
| Age of youngest child | 0.01       | 0.09    | 0.02    | 0.12    | 0.02    | 0.12    | 0.01        | 0.10    | 0.01    | 0.10   | 0.01    | 0.10   |
| Child tax credit      | 8.31       | 7.17    | 8.33    | 7.02    | 8.56    | 7.30    | 9.50        | 7.13    | 9.61    | 7.45   | 9.52    | 7.35   |
| State marginal tax    | 8.08       | 62.71   | 9.04    | 64.69   | 12.95   | 83.63   | 119.91      | 162.39  | 134.37  | 179.42 | 154.24  | 206.87 |
| Federal tax burden    | 0.22       | 2.68    | 0.23    | 2.66    | 0.31    | 2.57    | 2.39        | 3.11    | 2.51    | 3.39   | 2.56    | 3.61   |
| TANF receipt          | 15.24      | 196.56  | 28.73   | 574.04  | 35.98   | 568.94  | 232.37      | 1346.16 | 223.55  | 975.76 | 243.23  | 826.65 |
| Food stamp receipt    | 242.62     | 895.19  | 233.79  | 911.09  | 234.11  | 1044.50 | 101.46      | 633.92  | 102.06  | 656.11 | 98.00   | 642.91 |
| SSI                   | 563.63     | 1075.89 | 539.99  | 1079.84 | 515.65  | 1075.69 | 235.29      | 743.59  | 230.78  | 746.38 | 233.28  | 740.51 |
|                       | 2.31       | 0.52    | 2.29    | 0.52    | 2.29    | 0.52    | 2.30        | 0.52    | 2.28    | 0.52   | 2.29    | 0.52   |
| Number of obs.        | 889        |         | 1862    |         | 2889    |         | 1466        |         | 2954    |        | 4361    |        |

| More than one child   | First kink |         |         |         |         |         | Second kink |         |         |         |         |         |
|-----------------------|------------|---------|---------|---------|---------|---------|-------------|---------|---------|---------|---------|---------|
| Bandwidth<br>Variable | \$1,000    |         | \$2,000 |         | \$1,000 |         | \$2,000     |         | \$1,000 |         | \$2,000 |         |
| Number of dependents  | Mean       | SD      | Mean    | SD      | Mean    | SD      | Mean        | SD      | Mean    | SD      | Mean    | SD      |
| White                 | 2.53       | 0.84    | 2.53    | 0.84    | 2.49    | 0.76    | 2.49        | 0.79    | 2.49    | 0.79    | 2.49    | 0.79    |
| Black                 | 0.67       | 0.47    | 0.68    | 0.47    | 0.66    | 0.47    | 0.67        | 0.47    | 0.67    | 0.47    | 0.67    | 0.47    |
| Other                 | 0.27       | 0.45    | 0.27    | 0.44    | 0.28    | 0.45    | 0.28        | 0.45    | 0.28    | 0.45    | 0.28    | 0.45    |
| Age                   | 0.06       | 0.23    | 0.06    | 0.23    | 0.06    | 0.23    | 0.06        | 0.23    | 0.06    | 0.23    | 0.06    | 0.23    |
| Less than high school | 34.40      | 7.39    | 34.48   | 7.51    | 34.87   | 7.28    | 34.83       | 7.30    | 34.83   | 7.30    | 34.83   | 7.30    |
| H.S. degree           | 0.26       | 0.44    | 0.26    | 0.44    | 0.24    | 0.43    | 0.23        | 0.42    | 0.23    | 0.42    | 0.23    | 0.42    |
| Some college          | 0.40       | 0.49    | 0.40    | 0.49    | 0.41    | 0.49    | 0.42        | 0.49    | 0.42    | 0.49    | 0.42    | 0.49    |
| College degree        | 0.29       | 0.45    | 0.29    | 0.45    | 0.30    | 0.46    | 0.30        | 0.46    | 0.30    | 0.46    | 0.30    | 0.46    |
| Post grad             | 0.04       | 0.21    | 0.04    | 0.21    | 0.05    | 0.21    | 0.04        | 0.20    | 0.04    | 0.20    | 0.04    | 0.20    |
| Age of youngest child | 0.01       | 0.08    | 0.01    | 0.08    | 0.01    | 0.09    | 0.01        | 0.07    | 0.01    | 0.07    | 0.01    | 0.07    |
| Child tax credit      | 6.62       | 4.70    | 6.60    | 4.72    | 6.88    | 4.59    | 6.87        | 4.66    | 6.87    | 4.66    | 6.87    | 4.66    |
| State marginal tax    | 6.29       | 76.28   | 7.48    | 86.86   | 16.96   | 105.94  | 20.19       | 121.84  | 20.19   | 121.84  | 20.19   | 121.84  |
| Federal tax burden    | 0.05       | 3.23    | 0.14    | 3.61    | 1.85    | 3.31    | 1.89        | 3.39    | 1.89    | 3.39    | 1.89    | 3.39    |
| TANF receipt          | 25.49      | 570.84  | 28.50   | 546.28  | 20.65   | 146.46  | 34.16       | 371.42  | 34.16   | 371.42  | 34.16   | 371.42  |
| Food stamp receipt    | 387.11     | 1530.10 | 386.99  | 1468.49 | 205.69  | 957.53  | 220.64      | 1044.67 | 220.64  | 1044.67 | 220.64  | 1044.67 |
| SSI                   | 1150.18    | 1786.81 | 1169.15 | 1819.49 | 803.22  | 1488.93 | 786.10      | 1520.25 | 786.10  | 1520.25 | 786.10  | 1520.25 |
|                       | 2.31       | 0.53    | 2.29    | 0.53    | 2.28    | 0.54    | 2.28        | 0.54    | 2.28    | 0.54    | 2.28    | 0.54    |
| Number of obs.        | 1407       |         | 2701    |         | 1603    |         | 3060        |         |         |         |         |         |

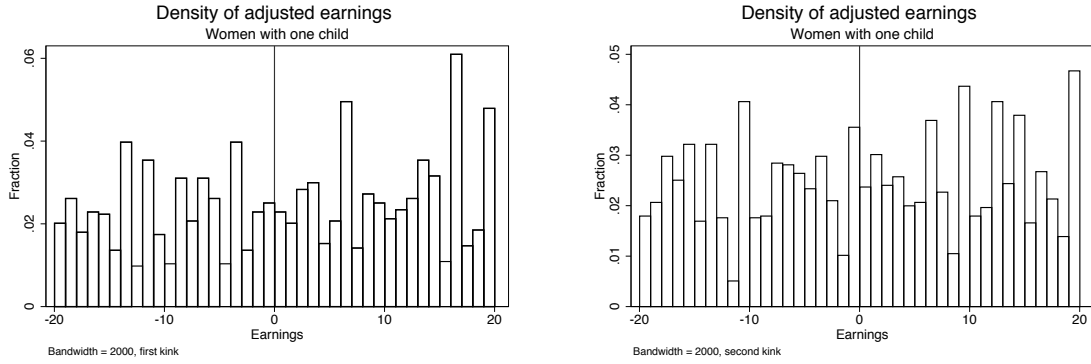


Figure 1.4: Histogram of earnings, women with one child.

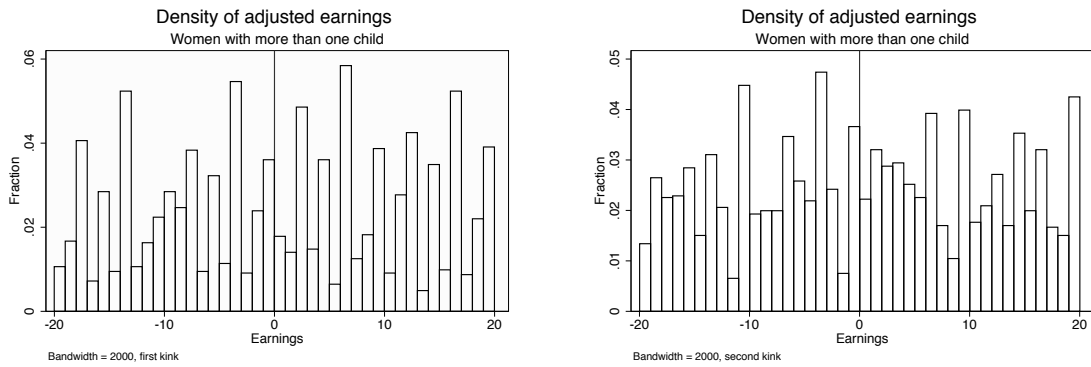


Figure 1.5: Histogram of earnings, women with more than one child.

A further requirement is that no covariates display kinks that correspond to the kink in the benefit function, which would indicate bias. This can be tested visually by looking at the distribution of covariates over bins of earnings.<sup>11</sup> While many covariates were tested in this manner—including demographic characteristics such as age, race, and so forth—the graphs included here are limited to those tax and benefit variables that seemed the most likely to confound the situation. These include the Child Tax Credit, state marginal tax rate, federal tax burden before credits, TANF and food stamp receipt, and Supplemental Security Income. These covariates will also be tested using seemingly unrelated regres-

<sup>11</sup>The choice of bin size (\$100 wide in real terms) was confirmed using a “bin test” prescribed by Lee and Lemieux (2010), which was ultimately agnostic within the \$2000 bandwidth regarding bin widths of \$200, \$100, \$50, or \$25.

sion, described later.

Even-numbered Figures 1.6 through 1.16 display the graphs for these variables at each kink for women with one child; the odd-numbered figures show the same for women with more than one child. For the sake of simplicity, only one bandwidth choice is shown: \$2000 on either side of the kink under consideration. First, receipt of the child tax credit appears not to differ for mothers with more than one child for either kink. For women with one child, there appears to be a steady increase in the Child Tax Credit that starts shortly before the second kink. State marginal tax rates appear to jump discontinuously at the first kink for both groups (but there appears to be no change in slope); for both groups at the second kink, state marginal tax rates appear to increase smoothly.

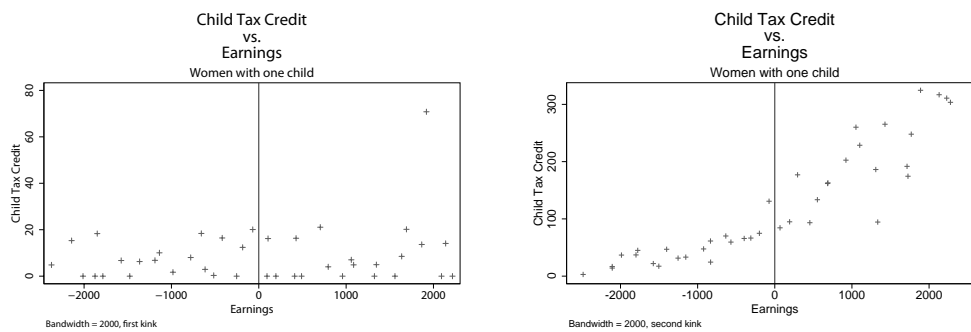


Figure 1.6: Child tax credit, women with one child.

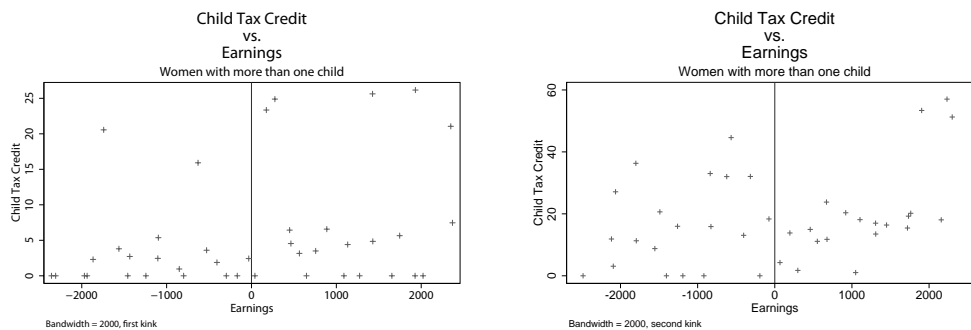


Figure 1.7: Child tax credit, women with more than one child

There is a sharp increase shortly before the second kink in federal income tax burden for women with one child. This sharp increase occurs because women with one child (assuming one exemption and one dependent) begin paying federal income tax once their total income is above \$15,000 in 2008 dollars. Thus, for women with one child, federal income tax liability amounting to 10% of taxable income kicks in shortly before the kink in the EITC where the benefit begins to decrease. Thus, it is likely impossible to disentangle a response to payment of federal tax with a response to the decrease in the EITC. For women with more than one child, the extra dependent exemption means that total income begins to be taxed at \$18,500, which is outside the earnings range under consideration. While there are clearly in the sample mothers with more than one child who have a small federal tax liability, which could be due to other taxable income besides earnings, the pattern of tax burden does not change at the kink.

In summary, a visual inspection of the covariates indicates that at the first kink for both groups, and at the second kink for the group with only one child, there exist covariates that may not meet the continuity requirements of the regression kink design. For women with more than one child whose earnings place them near the second kink, all covariates appear to be smooth. However, this will be tested formally.

In summary, a visual inspection of the covariates indicates that at the first kink for both

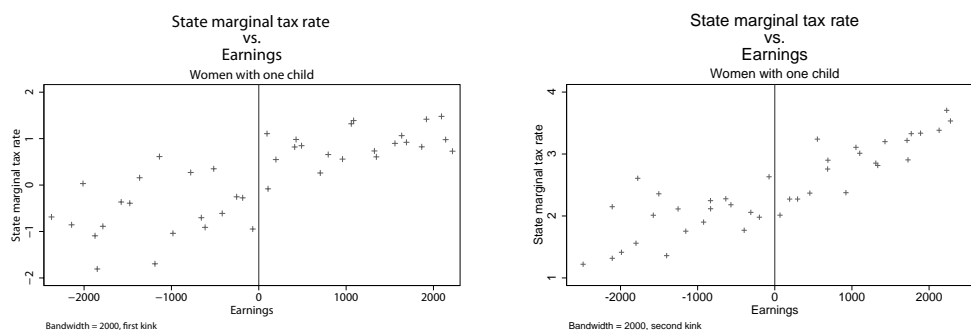


Figure 1.8: State marginal tax rate, women with one child.

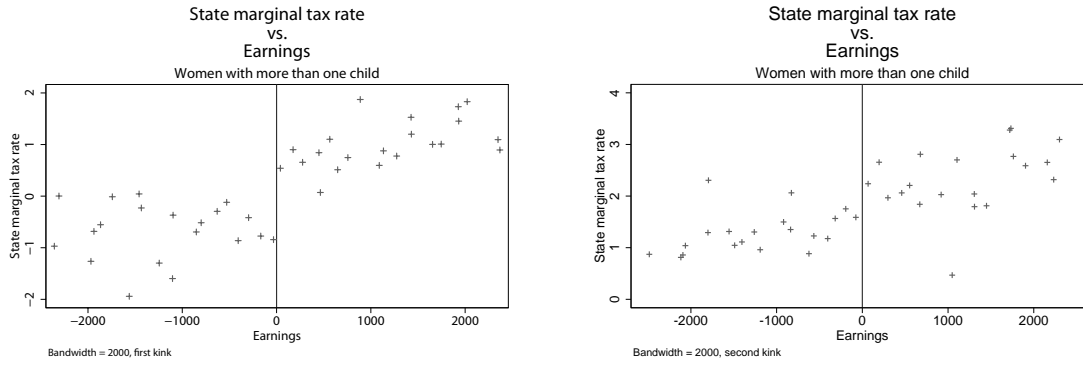


Figure 1.9: State marginal tax rate, women with more than one child.

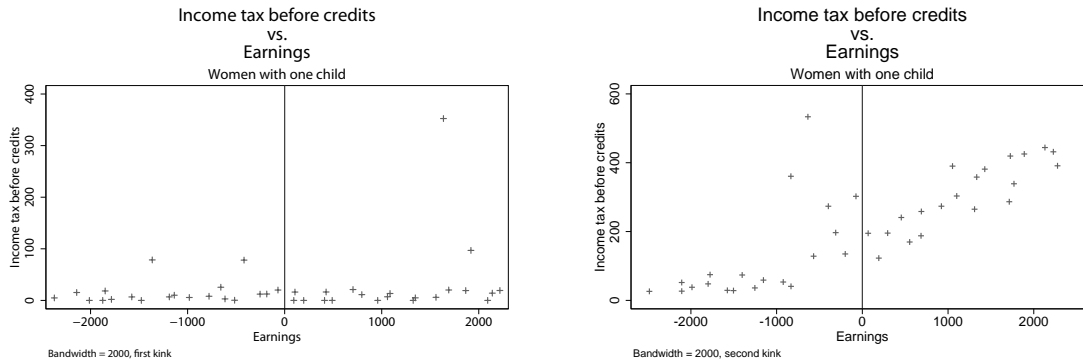


Figure 1.10: Federal tax burden before credits, women with one child.

groups, and at the second kink for the group with only one child, there exist covariates that may not meet the continuity requirements of the regression kink design. For women with more than one child whose earnings place them near the second kink, all covariates appear to be smooth. However, this will be tested formally.

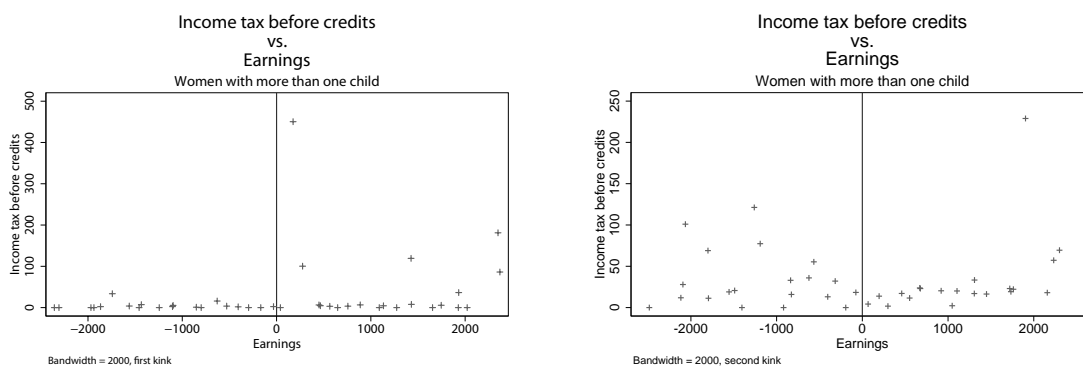


Figure 1.11: Federal tax burden before credits, women with more than one child.

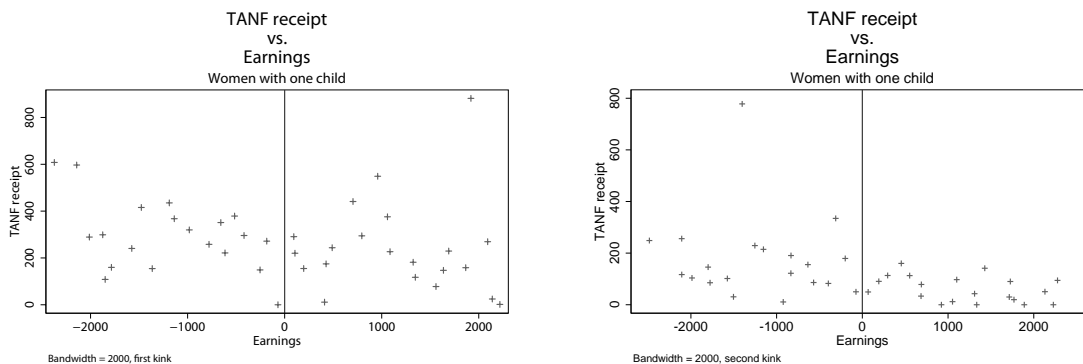


Figure 1.12: TANF receipt, women with one child.

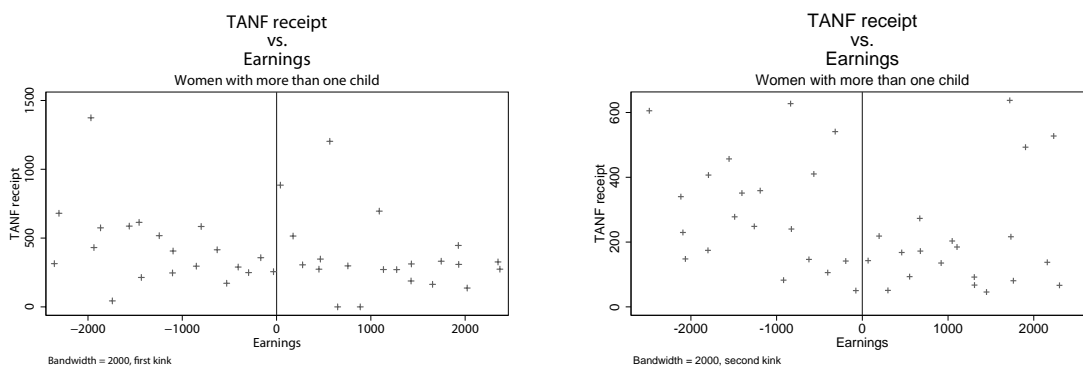


Figure 1.13: TANF receipt, women with more than one child.



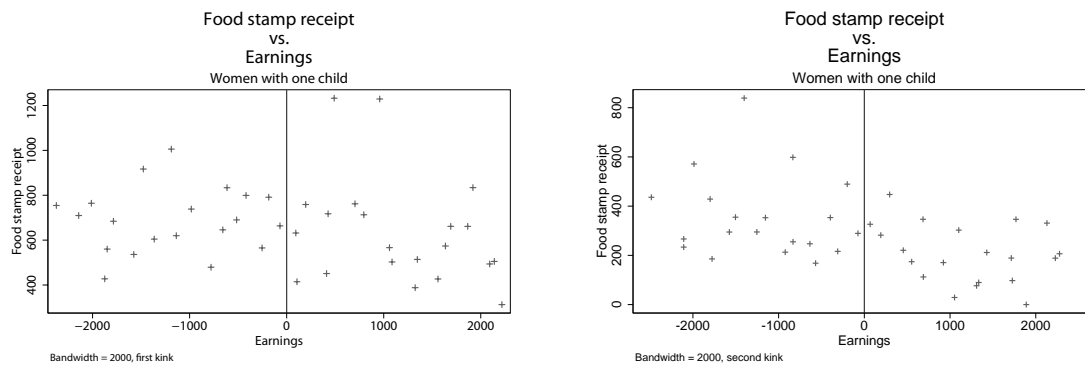


Figure 1.14: Food stamp receipt, women with one child.

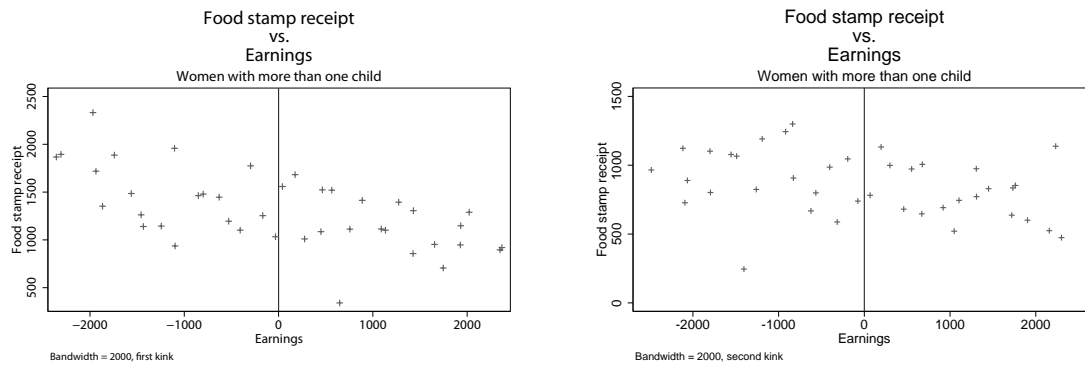


Figure 1.15: Food stamp receipt, women with more than one child.

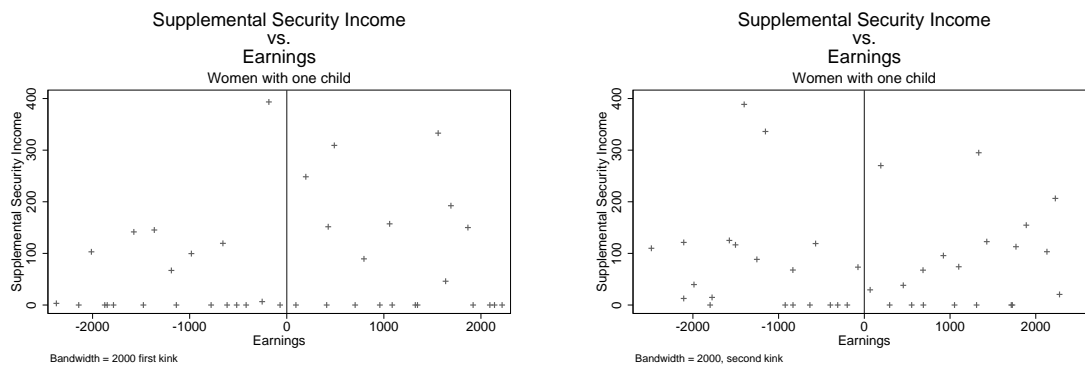


Figure 1.16: Supplemental Security Income receipt, women with one child.

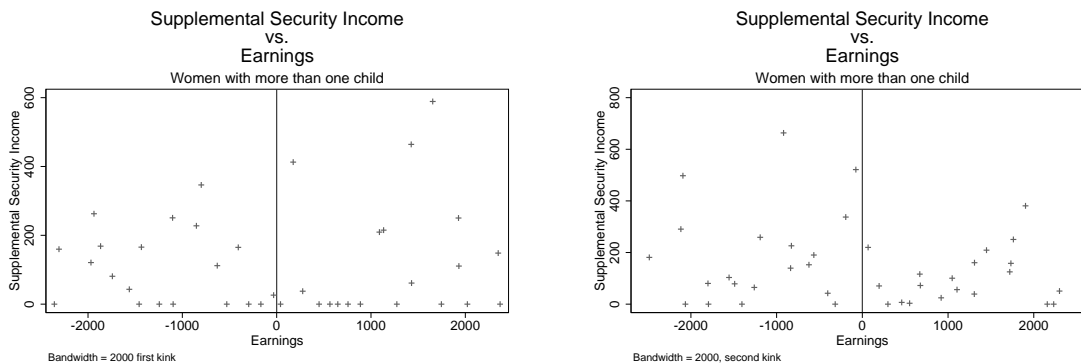


Figure 1.17: Supplemental Security Income receipt, women with more than one child.

## 1.5 Results

### 1.5.1 Graphs of benefit/hours relationship

For the sake of simplicity, I show graphs of the benefit-hours relationship for one bandwidth choice: \$2000 on either side of each kink. Figures 1.18 and 1.19 show the average number of hours worked per year as a function of income for women with one child at the first and second kink, respectively. Hours were binned over 40 equal-sized intervals of income (within the range of income that leaves one eligible for the EITC), and average hours are plotted for each bin. In both cases, upon visual inspection there appears to be no change in slope on either side of the kink, but this will be examined econometrically.

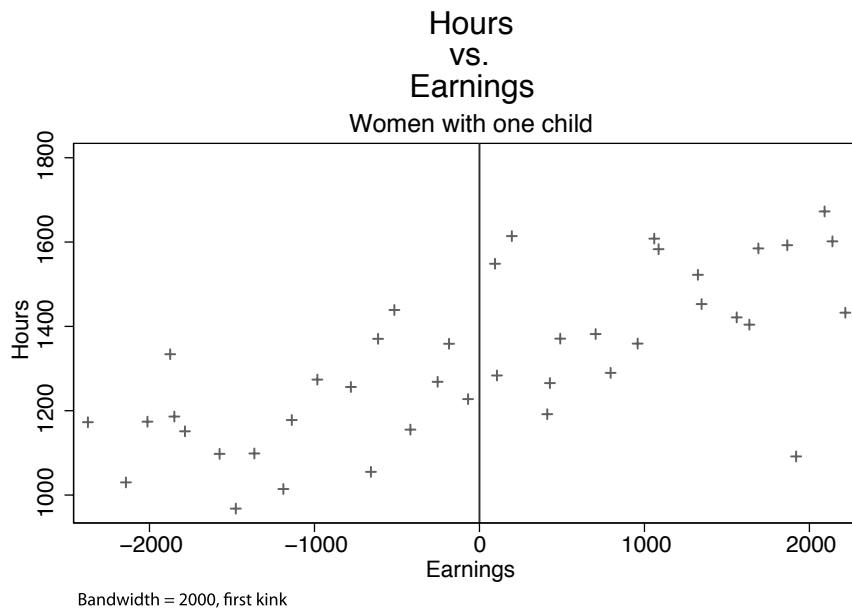


Figure 1.18: Hours worked vs. earnings, women with one child, first kink

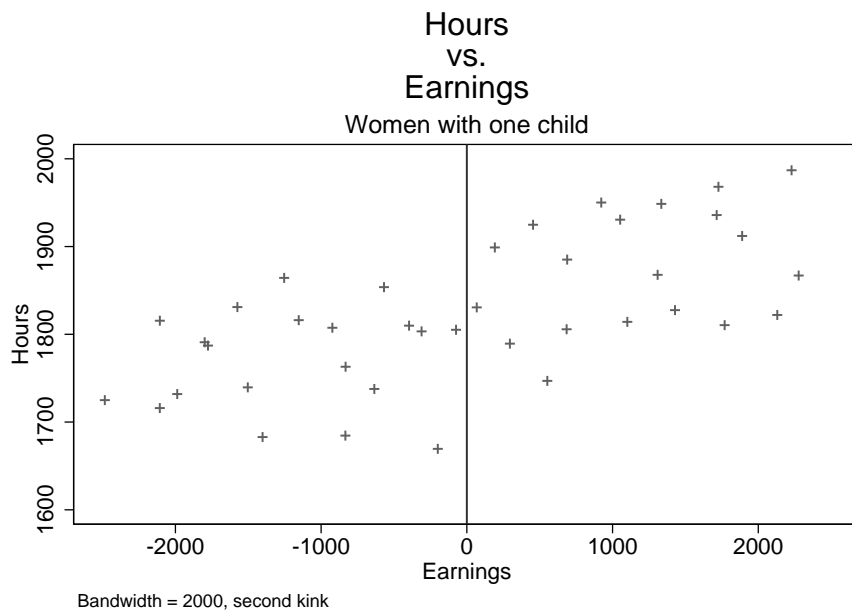


Figure 1.19: Hours worked vs. earnings, women with one child, second kink

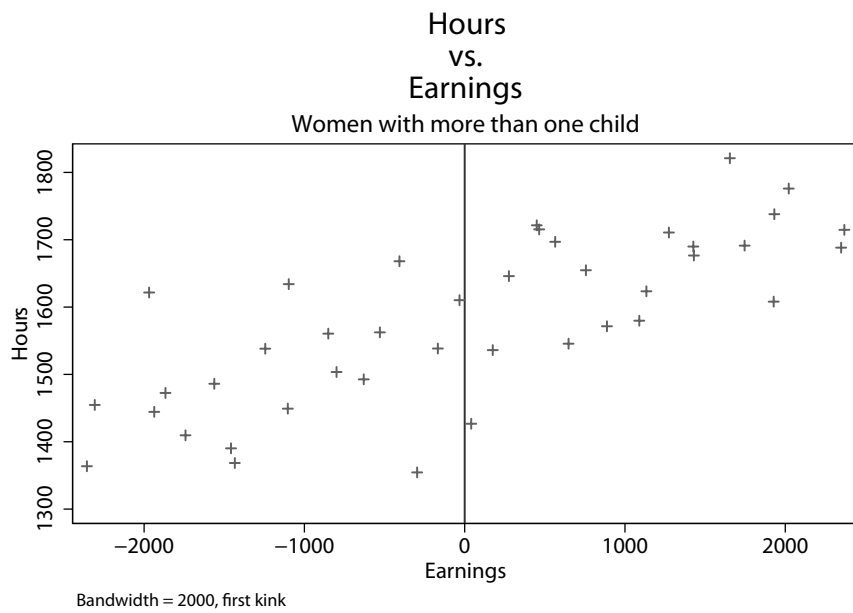


Figure 1.20: Hours worked vs. earnings, women with more than one child, first kink

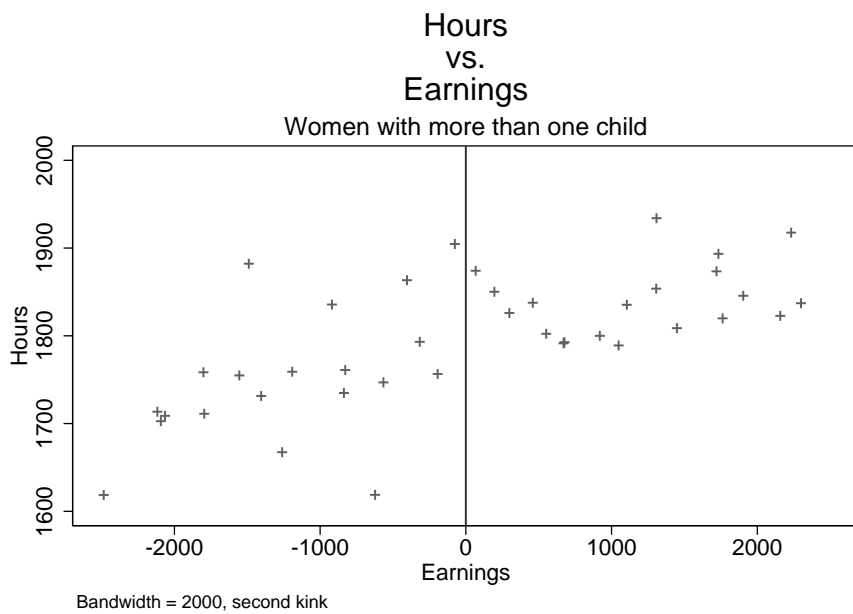


Figure 1.21: Hours worked vs. earnings, women with more than one child, second kink

Figures 1.20 and 1.21 show the same graphs for women with more than one child. At the first kink, there is evidence of a slight change in slope, but it occurs well beyond the actual kink point. At the second kink, an abrupt change of slope occurs immediately at the kink point, although hours worked does appear to recover \$1000 beyond the kink.

### 1.5.2 Tables of results

Table 1.2 shows the results of estimating a change in hours at the first kink. The odd-numbered equations are parsimonious regressions using only the slope terms. Even-numbered equations include year fixed effects. Although there are (marginally) significant estimates within the smaller bandwidth and higher-order polynomial terms for both groups of single mothers, these results disappear upon the addition of covariates. Moreover, none of the models with statistically significant results passed any of the robustness checks described below.

Table 1.3 shows the results at the second kink. For women with one child, the results also show no change in behavior, except in the largest bandwidth and first polynomial order. For women with more than one child, results are more convincing. Using a bandwidth of \$1000, estimates of the change in slope are significant for both the linear and second-order-polynomial model. Using the \$2000 bandwidth, results are significant for the third-order models. In all cases the coefficient has the same, and “expected,” sign (that is, hours appear to decrease). Looking at the Schwartz statistic for each case indicates that the second or third order is preferred in the smaller bandwidth, and the third in the larger. However, the difference in Schwartz statistics is quite small among the three orders. These estimates suggest that women who are eligible for a credit just beyond the kink point respond by decreasing their reported hours of work by between .14 and .80 of an hour per year. Further robustness checks indicate that the second-order polyno-

Table 1.2: Regression kink design estimates of change in hours worked, first kink

| Women with one child           |  | Poly Order<br>One |                   |                    | Poly Order<br>Two  |                    |                    | Poly Order<br>Three |  |  |
|--------------------------------|--|-------------------|-------------------|--------------------|--------------------|--------------------|--------------------|---------------------|--|--|
| Bandwidth 1000                 |  | 1                 | 2                 | 3                  | 4                  | 5                  | 6                  |                     |  |  |
| $\beta_1$                      |  | 0.040<br>(0.116)  | 0.056<br>(0.120)  | -0.972*<br>(0.419) | -0.913*<br>(0.443) | -2.147*<br>(1.016) | -1.983<br>(1.016)  |                     |  |  |
| Obs.                           |  | 889               |                   |                    |                    |                    |                    |                     |  |  |
| Bandwidth 2000                 |  |                   |                   |                    |                    |                    |                    |                     |  |  |
| $\beta_1$                      |  | 0.010<br>(0.039)  | 0.003<br>(0.039)  | -0.155<br>(0.150)  | -0.130<br>(0.155)  | -0.534<br>(0.360)  | -0.478<br>(0.372)  |                     |  |  |
| Obs.                           |  | 1862              |                   |                    |                    |                    |                    |                     |  |  |
| Bandwidth 3000                 |  |                   |                   |                    |                    |                    |                    |                     |  |  |
| $\beta_1$                      |  | -0.001<br>(0.021) | 0.001<br>(0.021)  | 0.069<br>(0.080)   | 0.064<br>(0.082)   | -0.386*<br>(0.190) | -0.413*<br>(0.193) |                     |  |  |
| Obs.                           |  | 2889              |                   |                    |                    |                    |                    |                     |  |  |
| Women with more than one child |  | One               |                   |                    | Two                |                    |                    | Three               |  |  |
| Bandwidth 1000                 |  | 1                 | 2                 | 3                  | 4                  | 5                  | 6                  |                     |  |  |
| $\beta_1$                      |  | -0.000<br>(0.080) | -0.001<br>(0.087) | 0.257<br>(0.288)   | 0.546<br>(0.345)   | 1.594*<br>(0.706)  | 1.619*<br>(0.789)  |                     |  |  |
| Obs.                           |  | 1407              |                   |                    |                    |                    |                    |                     |  |  |
| Bandwidth 2000                 |  |                   |                   |                    |                    |                    |                    |                     |  |  |
| $\beta_1$                      |  | -0.009<br>(0.028) | -0.018<br>(0.029) | 0.012<br>(0.110)   | -0.011<br>(0.114)  | 0.113<br>(0.253)   | 0.025<br>(0.263)   |                     |  |  |
| Obs.                           |  | 2701              |                   |                    |                    |                    |                    |                     |  |  |

Robust standard errors in parentheses. Odd-numbered models are parsimonious, using only the slope terms. Even-numbered models include year fixed effects. Each bandwidth choice expresses distance from the first kink in 2008 dollars. The dependent variable is hours worked per year.

mial model in the smaller bandwidth fits the data best, with the third order in the larger bandwidth the second-most preferred. Taking this estimate (an intent to treat effect) to be approximately  $-0.7$  hours, and assuming that all single mothers who are eligible for the EITC receive it, a treatment on the treated effect would be  $-0.7 * 4.75$ , or approximately  $-3.25$  hours per year.<sup>12</sup>

Further polynomial orders were attempted, but adding more terms did not improve the Schwartz statistic for the fourth or fifth polynomial specification, and by the fourth order, terms began to drop due to multicollinearity.

The remaining analysis proceeds on the second kink alone. Table 1.4 shows the results with the inclusion of the covariates, including race, age, education, number of dependents (for mothers with more than one), age of youngest child, and the tax and benefit variables described in section 4.2. The odd-numbered equations show the results with the covariates included in the parsimonious specifications, and the even-numbered equations include year fixed effects. While results for mothers with more than one child remain basically the same, the single significant coefficient for women with one child is no longer different from 0.

A conclusion that may be taken away from these results is that certain features of the tax and transfer program in the U.S. combine to confound the effect of the EITC at the first kink, and at the second kink for women with one child. State marginal taxes, federal income taxes and the Child Tax Credit all appear to coincide uncomfortably with the EITC benefit function in those cases. For women with two children whose earnings are near the second kink, no such policy confusion is discernible, at least in the observables.

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<sup>12</sup>The  $TT$  estimate, or RKD estimand, is  $\beta_1$  divided by the derivative of the benefit function (21.06%). Without knowing the true proportion of mothers in my sample and within the applicable bandwidth who receive the EITC, a true  $TT$  estimate cannot be calculated.

Table 1.3: Regression kink design estimates of change in hours worked, second kink

| Women with one child           |  | Poly Order<br>One  |                    | Poly Order<br>Two    |                    | Poly Order<br>Three |                      |
|--------------------------------|--|--------------------|--------------------|----------------------|--------------------|---------------------|----------------------|
| Bandwidth 1000                 |  | 1                  | 2                  | 3                    | 4                  | 5                   | 6                    |
| $\beta_1$                      |  | -0.006<br>(0.070)  | -0.029<br>(0.071)  | -0.145<br>(0.270)    | -0.052<br>(0.308)  | 0.552<br>(0.659)    | 0.552<br>(0.659)     |
| Obs.                           |  | 1466               |                    |                      |                    |                     |                      |
| Bandwidth 2000                 |  |                    |                    |                      |                    |                     |                      |
| $\beta_1$                      |  | 0.008<br>(0.024)   | 0.004<br>(0.025)   | 0.104<br>(0.093)     | 0.150<br>(0.095)   | -0.152<br>(0.225)   | -0.198<br>(0.226)    |
| Obs.                           |  | 2954               |                    |                      |                    |                     |                      |
| Bandwidth 3000                 |  |                    |                    |                      |                    |                     |                      |
| $\beta_1$                      |  | -0.028*<br>(0.014) | -0.033*<br>(0.014) | 0.021<br>(0.053)     | 0.049<br>(0.053)   | 0.223<br>(0.127)    | 0.209<br>(0.127)     |
| Obs.                           |  | 4361               |                    |                      |                    |                     |                      |
| Women with more than one child |  | One                |                    | Two                  |                    | Three               |                      |
| Bandwidth 1000                 |  | 1                  | 2                  | 3                    | 4                  | 5                   | 6                    |
| $\beta_1$                      |  | -0.138*<br>(0.064) | -0.153*<br>(0.066) | -0.799***<br>(0.245) | -0.684*<br>(0.300) | -0.434<br>(0.633)   | -0.572<br>(0.704)    |
| Obs.                           |  | 1603               |                    |                      |                    |                     |                      |
| Bandwidth 2000                 |  |                    |                    |                      |                    |                     |                      |
| $\beta_1$                      |  | -0.038<br>(0.023)  | -0.043<br>(0.024)  | -0.163<br>(0.088)    | -0.134<br>(0.089)  | -0.656**<br>(0.221) | -0.775***<br>(0.228) |
| Obs.                           |  | 3060               |                    |                      |                    |                     |                      |

Robust standard errors in parentheses. Odd-numbered models are parsimonious, using only the slope terms. Even-numbered models include year fixed effects. Each bandwidth choice expresses distance from the second kink in 2008 dollars. The dependent variable is hours worked per year.



Table 1.4: Regression kink design estimates of change in hours worked, second kink, including covariates

| Women with one child           |  | Poly Order<br>One  |                    | Poly Order<br>Two    |                    | Poly Order<br>Three |                      |
|--------------------------------|--|--------------------|--------------------|----------------------|--------------------|---------------------|----------------------|
| Bandwidth 1000                 |  | 1                  | 2                  | 3                    | 4                  | 5                   | 6                    |
| $\beta_1$                      |  | 0.005<br>(0.068)   | -0.006<br>(0.070)  | -0.171<br>(0.268)    | -0.137<br>(0.308)  | 0.263<br>(0.657)    | 0.263<br>(0.657)     |
| Obs.                           |  | 1466               |                    |                      |                    |                     |                      |
| Bandwidth 2000                 |  |                    |                    |                      |                    |                     |                      |
| $\beta_1$                      |  | 0.017<br>(0.024)   | 0.020<br>(0.025)   | 0.129<br>(0.092)     | 0.142<br>(0.093)   | -0.217<br>(0.220)   | -0.208<br>(0.222)    |
| Obs.                           |  | 2954               |                    |                      |                    |                     |                      |
| Bandwidth 3000                 |  |                    |                    |                      |                    |                     |                      |
| $\beta_1$                      |  | -0.018<br>(0.014)  | -0.017<br>(0.014)  | 0.050<br>(0.052)     | 0.058<br>(0.052)   | 0.199<br>(0.124)    | 0.201<br>(0.125)     |
| Obs.                           |  | 4361               |                    |                      |                    |                     |                      |
| Women with more than one child |  | One                |                    | Two                  |                    | Three               |                      |
| Bandwidth 1000                 |  | 1                  | 2                  | 3                    | 4                  | 5                   | 6                    |
| $\beta_1$                      |  | -0.132*<br>(0.063) | -0.146*<br>(0.064) | -0.770***<br>(0.237) | -0.611*<br>(0.289) | -0.481<br>(0.615)   | -0.497<br>(0.687)    |
| Obs.                           |  | 1603               |                    |                      |                    |                     |                      |
| Bandwidth 2000                 |  |                    |                    |                      |                    |                     |                      |
| $\beta_1$                      |  | -0.038<br>(0.023)  | -0.041<br>(0.023)  | -0.158<br>(0.086)    | -0.131<br>(0.087)  | -0.643**<br>(0.215) | -0.755***<br>(0.221) |
| Obs.                           |  | 3060               |                    |                      |                    |                     |                      |

Robust standard errors in parentheses. Covariates include race, age, education, age of youngest child, and receipt of TANF, food stamps, Supplemental Security Income, the Child Tax Credit, marginal state income tax, and federal income tax paid. Each bandwidth choice expresses distance from the second kink in 2008 dollars. The dependent variable is hours worked per year.

## 1.6 Specification and falsification tests

Using guidance from Lee and Lemieux (2010) and Card et al. (2009), I ran several tests to check the robustness of the results. Because results were robust only for the second kink, I restrict my report on the results of the tests for this case.

### 1.6.1 Polynomial/placebo test

As described in Lee and Lemieux (2010), I ran a test for the correct polynomial form as follows: I added sets of bin dummies to the regressions (a set of bins \$100 wide were chosen for each bandwidth). If a joint test of significance on the bin dummies rejects the null that the dummies are jointly 0, higher-order polynomial terms are added and the test is run again until a model can no longer reject the null. For the analysis at the second kink and for women with one child, no polynomial orders are rejected at the \$1000 bandwidth; all polynomial orders reject the null at the \$2000 bandwidth; and at the \$3000 bandwidth, the first and second orders reject the null and the third does not. In sum, the single result of interest for women with one child is rejected by the test.

In contrast, for women with more than one child, the first and second orders in the \$1000 bandwidth don't reject the null; the third order does. In the \$2000 bandwidth, the first and second order reject, and the third does not reject.

This test can also give insight into whether there are other discontinuities besides the cutoff in the regression function, since the test measures whether the coefficients on the bin dummies are the same (in other words, there is no discontinuity in the regression line at bin edges).

To further check that the change in slope in hours occurs uniquely at the kink, I created placebo cutoffs at \$10 increments over each bandwidth and checked that no abrupt

changes in slope occur at these cutoffs. Some change in slope may occur by random chance, but I considered the test a failure if more than 5% of the placebo coefficients were significant at the same level as the estimate at the true cutoff.

This test failed for the sample of women with one child, with a large percentage of the placebo coefficients being significantly different from 0 for each one of the bandwidth and polynomial choices. For women with more than one child, the analysis failed the test for the first polynomial order in the smaller bandwidth, with about 19% of the coefficients significant. For the second order in the smaller bandwidth, the analysis passed, with less than 4% of the coefficients significant. For the third-order polynomial in the larger bandwidth, the analysis failed the test, with about 8% of the placebo coefficients significant. The results of the foregoing indicate a preference for the second order within the smaller bandwidth.

### **1.6.2 Ineligibles**

A second test posits that a population similar to the one being considered, single mothers, should not experience a change in behavior at the kink point when they in fact are not eligible for a benefit. Using single women without children, I reran the analysis for the \$1000 and \$2000 bandwidth choice, using the second kink (in other words, I artificially gave this group of ineligible women the same “treatment” as women with children). The results are reported in Table 1.5. There is a single significant coefficient in the larger bandwidth when year fixed effects are included. However, the significance disappears when I add covariates to the model (results not shown).

Table 1.5: Regression kink design estimates of change in hours worked, second kink, using single women with no children

| Women with no children |  |                   |                  |                   |                   |                     |                   |
|------------------------|--|-------------------|------------------|-------------------|-------------------|---------------------|-------------------|
|                        |  | Poly Order<br>One |                  | Poly Order<br>Two |                   | Poly Order<br>Three |                   |
| Bandwidth 1000         |  | 1                 | 2                | 3                 | 4                 | 5                   | 6                 |
| $\beta_1$              |  | 0.051<br>(0.044)  | 0.031<br>(0.045) | 0.197<br>(0.172)  | 0.364<br>(0.194)  | -0.030<br>(0.425)   | -0.030<br>(0.425) |
| Obs.                   |  | 4727              |                  |                   |                   |                     |                   |
| Bandwidth 2000         |  |                   |                  | 1                 |                   |                     |                   |
| $\beta_1$              |  | 0.018<br>(0.015)  | 0.007<br>(0.015) | 0.081<br>(0.061)  | 0.121*<br>(0.061) | 0.143<br>(0.146)    | 0.161<br>(0.148)  |
| Obs.                   |  | 9167              |                  |                   |                   |                     |                   |

Robust standard errors in parentheses. Women are defined as having “no children” if no other person is dependent upon them. Odd-numbered models are parsimonious, using only the slope terms. Even-numbered models include year fixed effects. Each bandwidth choice expresses distance from the second kink in 2008 dollars. The dependent variable is hours worked per year.

### 1.6.3 Heaping

Another concern regards the assignment variable, since attributes of workers who round earnings may influence the dependent variable, leading to biased estimates (Barreca et al., 2010). As seen in the histograms of earnings, heaping is a definite problem in this case, and earnings in the CPS in general are well-known to have rounding problems. To check for bias, I created a dummy variable equal to 1 for those observations that are heaped (a count more than 20) and interacted this dummy with the slope terms. Results are reported in Table 1.6. While the estimates of  $\beta_1$  for the preferred specification are slightly larger in absolute value and less precisely estimated, they are qualitatively the same.

Using CPS earnings as the assignment variable, outside of the rounding issue, could present a problem if earners are not correctly assigned to true side of the kink point. In comparing CPS data to Social Security Administration data, low-earning men are more likely to over-report earnings and low-earning women to under-report. However, in general, systematic misreporting in earnings appears concentrated to very low earnings levels, and women report more accurately than do men (Bound and Krueger, 1989; Bollinger, 1998). As long as the women considered here are assigned to the correct side of the kink regardless of rounding, rounding itself does not appear to present a problem.

### 1.6.4 Seemingly unrelated regressions

Next, I checked more formally that no covariates display a difference in behavior at the kink point in the assignment variable. Having looked at the graphs, it is clear going into this analysis that some covariates do, indeed, experience a change at or near the kink point for women with one child. Most relevant of these are federal tax burden, state marginal tax, and the Child Tax Credit.

To formally test these covariates, I performed a seemingly unrelated regression anal-

Table 1.6: Regression kink design estimates of change in hours worked, controlling for heaping

| Women with one child           |      | Poly Order One      |                     |                      | Poly Order Two     |                    |                     | Poly Order Three |  |  |
|--------------------------------|------|---------------------|---------------------|----------------------|--------------------|--------------------|---------------------|------------------|--|--|
|                                |      | 1                   | 2                   | 3                    | 4                  | 5                  | 6                   |                  |  |  |
| Bandwidth 1000                 |      |                     |                     |                      |                    |                    |                     |                  |  |  |
| $\beta_1$                      |      | 0.039<br>(0.080)    | 0.002<br>(0.083)    | -0.240<br>(0.311)    | -0.120<br>(0.330)  | -0.062<br>(0.747)  | -0.062<br>(0.747)   |                  |  |  |
| Obs.                           | 1466 |                     |                     |                      |                    |                    |                     |                  |  |  |
| Bandwidth 2000                 |      |                     |                     |                      |                    |                    |                     |                  |  |  |
| $\beta_1$                      |      | 0.001<br>(0.028)    | -0.005<br>(0.029)   | 0.118<br>(0.109)     | 0.169<br>(0.109)   | -0.125<br>(0.263)  | -0.210<br>(0.265)   |                  |  |  |
| Obs.                           | 2954 |                     |                     |                      |                    |                    |                     |                  |  |  |
| Bandwidth 3000                 |      |                     |                     |                      |                    |                    |                     |                  |  |  |
| $\beta_1$                      |      | -0.041**<br>(0.015) | -0.048**<br>(0.016) | 0.005<br>(0.059)     | 0.027<br>(0.059)   | 0.243<br>(0.144)   | 0.222<br>(0.144)    |                  |  |  |
| Obs.                           | 4361 |                     |                     |                      |                    |                    |                     |                  |  |  |
| Women with more than one child |      | Poly Order One      |                     |                      | Poly Order Two     |                    |                     | Poly Order Three |  |  |
|                                |      | 1                   | 2                   | 3                    | 4                  | 5                  | 6                   |                  |  |  |
| Bandwidth 1000                 |      |                     |                     |                      |                    |                    |                     |                  |  |  |
| $\beta_1$                      |      | -0.123<br>(0.075)   | -0.152<br>(0.080)   | -0.986***<br>(0.298) | -0.826*<br>(0.325) | -0.961<br>(0.750)  | -0.992<br>(0.768)   |                  |  |  |
| Obs.                           | 1603 |                     |                     |                      |                    |                    |                     |                  |  |  |
| Bandwidth 2000                 |      |                     |                     |                      |                    |                    |                     |                  |  |  |
| $\beta_1$                      |      | -0.025<br>(0.025)   | -0.036<br>(0.026)   | -0.177<br>(0.102)    | -0.146<br>(0.102)  | -0.665*<br>(0.264) | -0.775**<br>(0.271) |                  |  |  |
| Obs.                           | 3060 |                     |                     |                      |                    |                    |                     |                  |  |  |

Robust standard errors in parentheses. A binary variable equal to 1 if an observation is “heaped” (see text) is interacted with the slope terms. Even-numbered models include year fixed effects. Each bandwidth choice expresses distance from the second kink in 2008 dollars. The dependent variable is hours worked per year.

ysis as recommended by Lee and Lemieux (2010) for each specification shown in Table 1.4, using the same covariates (results for the models of interest are shown in Table 1.7). Reported are the coefficients for each of the tax and transfer variables. Also included were the demographic characteristics included in the covariate model. For women with more than one child, an F test failed to reject the null hypothesis that all coefficients were jointly equal to 0 except in the case of the larger bandwidth and first polynomial ( $\text{Prob} > \text{Chi}^2 = 0.011$ ). For women with one child, several specifications yielded F tests that rejected the null, including the largest bandwidth and first order that is of interest.

## 1.7 Conclusion

The analysis presented here examined the intensive-margin labor supply response of single mothers to the EITC using a regression kink design. For women with more than one child, the preferred specification indicated that women just beyond the kink in the EITC benefit function where the benefit begins to decrease responded by reducing hours of work by approximately .7 of an hour per year. This estimate withstood several robustness checks, although other polynomial specifications did not. No effect that withstood robustness checks was found for women with only one child, and no effect was found for either group at the kink in the benefit function where earners enter the plateau.

More than one argument could be made to explain these results. In regards to entering the plateau region, arguments that have been made in the past may hold true here: such low earners may have no say in the number of hours they work, but must rely on labor-market norms and expectations. Another argument may be that there is simply no income effect: Workers at this low a level of earnings may wish for more hours of work regardless of receiving the maximum benefit if they perceive the benefit as a reward for work and not as an hourly wage subsidy.

Table 1.7: Seemingly unrelated regression estimates

|                     | Women with one child | Women with more than one child |         |         |
|---------------------|----------------------|--------------------------------|---------|---------|
|                     | 3000                 | 1000                           | 2000    |         |
| Bandwidth           | 1                    | 1                              | 2       | 3       |
| Polynomial          | -0.013               | 0.272                          | 0.462   | 0.987   |
| TANF                | (0.020)              | (0.142)                        | (0.523) | (0.518) |
| Food stamps         | -0.015               | 0.063                          | 0.497   | 0.508   |
|                     | (0.022)              | (0.225)                        | (0.830) | (0.743) |
| SSI                 | 0.004                | -0.023                         | -1.446* | -1.008  |
|                     | (0.021)              | (0.162)                        | (0.597) | (0.573) |
| CTC                 | 0.054***             | 0.001                          | 0.086   | 0.014   |
|                     | (0.005)              | (0.015)                        | (0.056) | (0.057) |
| State income tax    | 0.000                | 0.000                          | 0.000   | -0.002  |
|                     | (0.000)              | (0.000)                        | (0.002) | (0.002) |
| Federal income tax  | 0.052*               | 0.043                          | -0.010  | 0.045   |
|                     | (0.023)              | (0.025)                        | (0.090) | (0.182) |
| Chi2 test           | 150.020              | 19.54                          | 23.48   | 20.93   |
| P <sub>i</sub> chi2 | (0.000)              | (0.191)                        | (0.074) | (0.140) |

Robust standard errors in parentheses. Models reported are those with statistically significant estimates for  $\beta_1$  in the main results. Models include race, age, education, age of youngest child, and number of dependents.



At the second kink, working mothers face a steep marginal tax. Earners in the phase-out region lose 21.06 cents for every extra dollar they earn if they have two or more children, and 15.98 cents if they have one. Combined with payroll tax and federal and state income tax, phaseout-region taxpayers can face a marginal tax that exceeds 50% of their income (Liebman, 1998). It is not surprising, then, that any hours effect would be discernible here. Why women with two children appear to respond when women with one child do not might be explained by other tax and transfer policies that combine to confound the effect for the latter group.

However, there is always the possibility that these two groups value “leisure” differently. For single mothers, “leisure” is most likely to have the meaning “work at home”: housework, childcare, and so forth. The trade-off with hours of work involves time foregone raising children and the cost of childcare. One would expect this trade-off to become more expensive, psychologically and monetarily, with each additional child. Perhaps the difference between these groups of women indicates that a substitution effect, albeit measured at a later time than the earnings decision, does exist. If this is the case, the EITC expansion in 2009 that created a more generous schedule for families with three children likely cushioned the trade-off between paid work and work at home. Other changes to the EITC that have been suggested include instituting a less distortionary phase-out schedule (for example, phasing out multiple-children families at the same rate as single-child families) and providing a joint EITC/Child Tax Credit. However, it should be noted that while the reported coefficients are statistically significant, they are economically small. For any change in benefit structure, the cost of the change may outweigh any positive inducement for further hours worked.

The policy implications of the results are defensive in nature. The EITC has come under recent attack as being too generous, with the argument being that the credit is a form of welfare that induces less work. For example, Rep. Michele Bachmann (R-

MN) included the elimination of the Earned Income Tax Credit as part of her presidential platform.<sup>13</sup> Yet it has been demonstrated that the EITC induces labor force participation, and the results found here show that extra income, per se, does not dampen participation. The very small effect on hours occurs due to the phase-out, and at an upper bound of -3 hours, economically irrelevant.

This analysis contributes to the literature by providing evidence that some single mothers reduce their hours of work in response to what they receive in EITC. It also contributes to a train of inquiry into behavioral responses to updated information. The main limitations to the analysis presented here are the extent to which earners are truly able to update their information regarding the EITC, plus certain undesirable features of the data, including uncertainty regarding EITC receipt and the accuracy of reported earnings. However, the results of the study are suggestive of an hours effect for a group whose labor-market attachment is of great interest to policymakers, and further research using a RKD approach on better data may shed even more light on the topic.

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<sup>13</sup><http://thinkprogress.org/economy/2011/11/03/361033/bachmann-eliminate-tax-credit-poverty/?mobile=nc>, accessed April 28, 2012.

## CHAPTER 2

### RESPONSES TO LIVING WAGES: WAGES, FIRM LOCATION AND EXIT, AND EMPLOYMENT

#### 2.1 Introduction

The deteriorating minimum wage and increased cost of living in American cities led more than 60 municipalities to adopt living wage ordinances in the late 1990s and early to mid-2000s. These ordinances grew mainly out of a concern for equity, motivated by city leaders unwilling to accept that employees who work for the city, either directly or through contracting, would live in poverty. The first city to pass a living wage, Baltimore, did so after the discovery that full-time workers on city contracts were resorting to using soup kitchens to get by (Niedt et al., 1999).

A living wage ordinance may be adopted by a city, county, university, public school system, or other entity such as a housing authority or a commission. Empirical research had focused on cities, which have adopted ordinances that cover employees who work for the city directly or under contract, or who work for a business that receives a tax break or subsidy from the city, or both. Ordinances vary in their wording, coverage, and enforcement, one of the reasons why living wages present a challenge to researchers.

Another issue is that a given living-wage policy is likely similar to others enacted in nearby cities; the required wage level is likely related to local economic characteristics that also affect firm behavior and employment; and living-wage ordinances are related to one another and to local markets temporally. This spatial and temporal correlation has been investigated recently in regards to minimum wages (Dube et al., 2007); this paper adds to the research on living wages by adopting a similar analytic strategy.

This paper also adds to the research by looking into a neglected issue in the living

wage and minimum wage literature—that of firm location choice or exit in the face of a wage floor. Because living wages have limited geographic and industry application, they are likely easier to avoid, and the question of number of firms in a labor market becomes more critical than with a minimum wage.

The remainder of this paper is organized as follows: Section 2.2 provides background on living wage policies and covers the relevant literature. Section 2.3 describes the identification strategy used. Section 2.4 introduces the data and discusses the process by which living wage information was gathered and checked. Section 2.5 presents and discusses results, and Section 2.6 reports on the results of a falsification test. Section 2.7 concludes.

## **2.2 Background**

### **2.2.1 Living wage policy**

In the late 1990s and early 2000s, living wage ordinances became a popular way for municipalities to ensure those employees who worked for the city, or under a city contract or subsidy, received wages that raised them to or above the federal poverty line. Living wages are similar to minimum wages in the sense that they set an absolute minimum that employers must pay. Many cities have set living wages well above the state or federal minimum wage in recognition that the cost of living in urban areas is rising, while wages for low-skilled work continue to erode. Whether or not a city adopts a living wage is associated with other features of the labor market environment—cities that have high rates of unionization, income inequality, unemployment, and Black and Hispanic populations tend to pass living wage ordinances more often than do cities without these characteristics (Levin-Waldman, 2008).

To the extent that cities vary in their cost of living and labor environment, so too

do living wage ordinances. Early forms of ordinances (and the more common type)—contractor-only laws—apply only to firms that contract with the city for a variety of municipal services, including janitorial and fleet maintenance. Other ordinances require a living wage for private businesses that receive subsidies or tax breaks from a city (usually of a certain magnitude). Still other ordinances combine both types of coverage (Lester, 2011).

Living wages vary dramatically in their levels as well, with some living wages a couple dollars higher than the federal minimum wage (for example, Santa Fe, at \$8.50 in the mid-2000s) to nearly twice the minimum (Santa Barbara, at \$14.71 in 2006). Neumark and Adams (2003) identify the industries most likely to be covered under a living wage law. These include construction, transportation, communications, sanitary services, custodial services, protective services, and parking. Other industries likely to be covered by tax- and subsidy-relevant ordinances are businesses such as “big box” stores, chain restaurants, and hotels.

Moreover, updates to living wage rates are occasionally written into the ordinance, or rates change according to new policy-making on the part of a city’s government. Thus living wages may change in an orderly way (for example, a wage may be set at the rate necessary to keep a family of four above poverty with one full-time worker), or they may change at the discretion of a city employee in charge of tracking the living wage or of a municipal board. This aspect of living wages brings a level of uncertainty into any model of firm response.

All aspects of living wage ordinances mentioned above should make them more liable to avoidance by firms than minimum wages. First, they are set quite a bit higher than minimum wages, so firms that face coverage have a higher incentive to locate elsewhere. Second, they exist within a narrow geographic boundary—one that contains in most cases a central city but does not include adjacent urban areas that belong to another

municipality. Thus firms that want to locate within a labor market that includes a living-wage city may simply choose a location immediately outside the city's boundaries. Lester (2011) described two ways in which firms might react to living wage laws, either directly or indirectly. Those that are directly affected currently are located within the city or are planning a move there; fit an industry characteristic that makes them likely to be covered; and pay wages to their workers that fall below the living wage. Firms that are indirectly affected simply react to a perceived business climate that is signaled by the enactment of an ordinance. I limit my analysis to those employers who are the most likely to be directly affected.

Certain features of living wages make them difficult to analyze. No public-use economic data exist that uses the city as the unit of observation. Much of the work on living wages has used the CPS Outgoing Rotation Groups, which includes Metropolitan Statistical Area and, as of 2002, FIPS county codes. But living wage ordinances are only applicable within a city's borders. Further complicating matters is the spatial correlation of ordinance adoption. Adams and Neumark (2005b) found that wage and employment effects are stronger in living wage cities when a city nearby also has an ordinance, suggesting that taking regional heterogeneity into account is an important consideration. A final feature of living wages to consider is enforcement. Early ordinances did not include sanctions or other repercussion against businesses covered by the living wage that did not pay it. Ordinances written later, or those that were amended over time, are more likely to contain enforcement wording. Thus any longitudinal analysis of living wage must take serial and time autocorrelation into account.

### 2.2.2 Literature review

There is an extensive literature on the impact of minimum wages, much of it contentious. To begin with theory, which would also apply to living wages, the simple answer to an increase in the minimum wage is that “buyers” of labor will want less of it if its price is higher. Until the 1990s, a vast body of research found that minimum wages reduce employment, with estimated effects of anywhere from a -0.05 to -3.00 percent change in employment induced by a 10 percent increase in the minimum wage (Brown et al., 1982). Competing theories gained traction with new empirical work, with the focus turning to monopsony models or models with search frictions (Boal and Ransom (1997); Dickens et al. (1999); Card and Krueger (1995)). Unlike the perfectly competitive market models that predict negative employment following from the application of a binding minimum wage, the monopsony model (whether simple or dynamic) predicts ambiguous results that depend on a variety of factors, including how high the wage is set in comparison to the monopsony wage and the wage predicted by a competitive market.

A prediction of minimum wage theory that has been largely unexplored by researchers is that of firm avoidance or exit. Rebitzer and Taylor (1995) predict that in the longer run, if an increase in wages causes a firm to operate where profits are below zero, the firm will ultimately exit. As firms exit, unemployment increases. Very little empirical research exists that tests this prediction. One recent article is Carneiro and Portugal (2008), who look at the effect of a minimum wage in Portugal on firm exit. They found that a 10 percent increase in the number of minimum wage workers in a firm raised the probability of firm exit by 0.6 percent. In contrast, work by Draca et al. (2008) found that the U.K. national minimum wage was not associated with firm exit.

Living wage research has followed a couple of paths. The first is survey or cost-benefit studies that have examined firm responses to living wages in regards to contracting.

These include Schoenberger (2000), who focused on the costs of contracts to the city of Baltimore after its passage of a living wage law. In a similar vein, Reich et al. (1999) studied city contracts in San Francisco, and determined that low-wage workers in the city would earn dramatically more in wages and health benefits every year. Other city-based studies have examined how firms might handle the increased cost of labor. Pollin et al. (2002a), in a pre-ordinance survey of New Orleans-based businesses, determined that the cost of a living wage could be absorbed by businesses in the form of price and productivity changes and wage redistribution within the businesses; however, it should be noted that this study was projective in nature and not empirical.

Another field of study has focused on the characteristics of living-wage cities. Levin-Waldman (2008) used the CPS to compare cities that enact a living wage with those that do not, finding that cities in states with a high union density, and with higher income inequality, larger immigrant populations, and a larger proportion of African-American residents were more likely to enact living wage ordinances. A similar study by Gallet (2004) found that city-level annual per capita manufacturing earnings, fair market rent, proximity to another living wage city, and a state minimum wage level above the federal level had significantly positive effects on whether a city adopts a living wage. Other factors, such as characteristics of cities in terms of race and age, did not appear to have an effect. Swarts and Vasi (2011) found that larger cities tend to adopt living wages, as well as those with larger proportions of democratic voters, a history of progressive activism, and a larger density of progressive organizations.

Most nation-wide studies on the impact of living wages on employment takes as a starting point similar research on minimum wages. David Neumark and colleagues (Neumark, 2002; Neumark and Adams, 2003; Adams and Neumark, 2005b,c) have consistently found that cities which pass living wage ordinances see increases in wages (an estimated elasticity for wages of about 0.07) and decreases in employment (an elastic-



ity of about -0.15) for workers in the lowest decile of the wage distribution. For these studies, the authors use CPS data and a fixed-effects approach to estimate a difference in living-wage and non-living-wage Metropolitan Statistical Areas over time. One innovation the authors have employed is to use cities which had failed living wage campaigns as the control group for cities whose campaigns were successful (Adams and Neumark, 2005a); their findings were similar to their previous findings using this more convincing methodology.

In contrast, Lester (2011) used the National Establishment Time Series (NETS) to investigate the effects of living wage laws in California. These data have the benefit of having the city as the unit of observation,<sup>1</sup> but suffer from not including information on wages. While Lester found some evidence of decreased employment and establishments in living-wage cities, these disappear when living-wage and non-living-wage cities are matched on propensity scores. Fairris (2005) used a quasi-experimental approach to analyze the living wage law in Los Angeles, surveying firms and workers who were covered by the ordinance and those who were not. Employing a difference-in-differences design to compare the two groups, Fairris found that wages increased in covered firms, but employment did not decrease, relative to the control group. Moreover, absenteeism and turnover dropped in covered firms.

The research on living wages thus far therefore suffers from the same lack of consensus seen in minimum wage research, compounded by the absence of good national data on cities, on covered and non-covered workers, and on ordinance enforcement. Moreover, the research confirms that living wage cities are related to one another in space and time, pointing to the need to control for spatial heterogeneity. This study attempts to address some of the issues arising from previous research.

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<sup>1</sup>Unfortunately, the data are not publicly available.

## 2.3 Identification Strategy

Living wages present a problem in that they are applied over a small geographic area and for a limited set of industries. I use a fixed-effects approach that controls for ever smaller—and therefore ever-more homogeneous—geographic areas to compare places affected by a living wage with those not affected within larger areas that share a common labor market. The final stage of the analysis compares urban county pairs: pairs of counties that share a border and have within them an urban area of comparable density, where one urban area has a living wage in place and the other does not. The technique is similar to that used by Dube et al. (2007), with the exception that contiguous counties are further matched based on the existence of an urban area within them. In each specification, I control for both the living wage and the higher of the federal or state minimum wage, since it would otherwise be impossible to determine whether differences are induced by a living wage or by between-state variation in minimum wages in counties that cross state borders.

My outcomes of interest are wages, number of establishments, and employment. Number of establishments is of particular interest, as this has been a neglected area in minimum wage investigations. I first estimate parsimonious wage, establishment, and employment effects by using the full sample of U.S. counties, leaving out the states of Alaska and Hawaii. These models include county and time fixed effects, which capture the differences between living-wage counties and non-living-wage counties over time. The second specification includes Census-defined region effects multiplied by time to capture time-varying effects within each geographic division in the U.S. The third specification controls for time-specific metropolitan statistical region, which captures differences between living wage and non-living wage counties that are defined for the same metro region. Fourth, I pair up living-wage counties with their neighbors, creating county pairs

that share a border. Finally, I use only those county pairs that share a border and also have a Census-defined “urban area” within the county. This final specification recognizes that living-wage ordinances are most commonly passed within a city boundary, and that contiguous non-urban counties may not constitute a reasonable comparison.

Because both minimum wages and living wages may have effects that are lagged, I also estimate the models described above with a 2-quarter lag added to each specification. The lagged models are particularly relevant for estimating living wage effects, since the application of the living wage may be dependent on various types of contracting that may take some time to negotiate.

The monopsony model predicts three distinct “regimes” resulting from the setting of a minimum wage. If the minimum is set below the monopsony wage, no change in wage or employment results. If the minimum is set above the monopsony wage but below the competitive wage, the profit maximizing employment and wage will coincide with the labor supply curve (in other words, assuming that the supply curve is upward sloping, employment will increase). If the minimum is set above the competitive wage, the firm’s profit maximizing decision will lie along the marginal revenue product curve (in other words, employment may decrease)(Boal and Ransom, 1997). I predict, considering how much higher living wages are set compared with minimum wages, that living wages might be set above competitive wages, and will result in lower employment and firm exit or non-entry.

### 2.3.1 Models

The first model is a standard fixed-effects model using only the county and time fixed effects:

$$\ln(w_{it}) = \alpha_{it} + \beta * \ln(w_{it}^{min}) + \gamma * \ln(w_{it}^{liv}) + \phi_i + \eta_t + \epsilon_{it} \quad (2.1)$$

where  $w_{it}$  is the average wage per quarter for an industry in the set of industries likely covered by a living wage;  $w^{min}$  is the higher of the state minimum wage and the federal minimum wage; and  $w^{liv}$  is the higher of the state minimum wage and the city living wage. The fixed effects included are expressed by  $\phi_i$ , an indicator for each county, and  $\eta_{rt}$ , an indicator for each quarter. This specification has been used in the past under the assumption that the time and location dummies control for differences in labor market conditions. However, as pointed out in Dube et al. (2007) and in Chapter 3, local markets conditions may be correlated with the establishment of minimum and living wages, and this will create omitted variable bias that cannot be accounted for using the basic fixed-effects model.

Omitted variable bias is addressed using the next four sets of specifications, beginning with Census-defined region:

$$\ln(w_{it}) = \alpha_{it} + \beta * \ln(w_{it}^{min}) + \gamma * \ln(w_{it}^{liv}) + \phi_i + \eta_{rt} + \epsilon_{it} \quad (2.2)$$

In this specification, the  $rt$  subscripts indicate the Census-defined region multiplied by time, which captures only the variation in minimum wages and living wages occurring within regions over time. For this specification, the Central Southern region of the U.S. drops out, since there is no variation in state minimum wages or living wages over time in the entire region. This region experienced stronger employment overall than other areas of the U.S. during the time period in question (Dube et al., 2007) and its inclusion likely leads to estimates of minimum and living-wage effects that are biased.

The third specification becomes more local, controlling for Core Based Statistical Area multiplied by time:

$$\ln(w_{it}) = \alpha_{it} + \beta * \ln(w_{it}^{min}) + \gamma * \ln(w_{it}^{liv}) + \phi_i + \eta_{mt} + \epsilon_{it} \quad (2.3)$$

For this specification,  $\eta_{mt}$  controls for between-CBSA variation in employment, wages, and establishments, looking only at variation that occurs within a Census-defined metropolitan area (CBSA). CBSA designations generally include several counties, with one or more counties a “core urban area,” and other counties having a high degree of integration (social and economic) with the core area. In this case, all counties that are not part of a metropolitan area are dropped, and metropolitan areas that do not include a county with a living wage are also dropped, leaving only those metro area that contain a city or cities that have adopted a living wage. Living wage counties within metro areas constitute the variation in living wages, and metro areas that cross state borders constitute the variation in minimum wages.

The third specification pairs up counties, with a living-wage county paired with each adjacent non-living-wage county in turn.

$$\ln(w_{it}) = \alpha_{it} + \beta * \ln(w_{it}^{min}) + \gamma * \ln(w_{it}^{liv}) + \phi_i + \eta_{ct} + \epsilon_{it} \quad (2.4)$$

In this case,  $\eta_{ct}$  is a time-specific pair effect, and the variation in question is the difference in wage floor within each county pair. Because each living wage county appears in the data multiple times, the specification is weighted using the inverse of a county’s frequency in the sample.

This specification is the closest in spirit to that employed by Dube et al. (2007) using minimum wages, in that every county that experiences a “wage gradient” (in the living wage) across a county border is included in the sample. In the case of living wages, this specification is problematic, as there are many rural counties that border counties with a large urban area. Since urban counties are far more likely to experience higher wages and employment over time, especially in industries that contract with municipalities, the comparison group in this specification is likely inadequate. I take the procedure outlined

in Dube et al. (2007) a step further, and limit the sample to only urban counties.

Thus, the fourth specification pairs up adjacent counties again, but this time only pairs counties that include a Census-defined urban area.

$$\ln(w_{it}) = \alpha_{it} + \beta * \ln(w_{it}^{min}) + \gamma * \ln(w_{jt}^{min}) + \phi_i + \eta_{ut} + \epsilon_{it} \quad (2.5)$$

The urban area in question might be part of a living-wage city's metro area that crosses the border into an adjacent county, or it might be a completely unrelated urban area. This specification has the advantages of the preceding version in terms of controlling for spatial heterogeneity, but matches counties more closely on their labor market environment. The model is again weighted by the number of times a county appears in the data.

Because minimum and living wages may have a delayed effect on employment ((Neumark and Adams, 2003)), I also run the models described in the preceding section but include minimum and living wages lagged by 2 quarters (6 months). In the case of living wages, checking for lagged effects seems particularly important, as firms that contract with a city or receive a subsidy may respond to the enactment of a living wage well after its enactment or amendment.

Dube et al. (2007) demonstrate that, when using a fixed-effects approach and panel data, standard errors may not be consistent due to both temporal and spatial autocorrelation. They also point out that this type of autocorrelation can be even more of a problem if "correlation in the residuals is accompanied by a parallel correlation in the treatment variable across observations." This issue is of particular concern with living wages, since the enactment of a living wage is correlated with whether a nearby city has a living wage. I therefore cluster standard errors at the different geographic levels used in the controls: at the county for model 1, the region for model 2, the CBSA for model 3, and the group of counties making up pairs for models 4 and 5.

## **2.4 Data, Sample, and Descriptives**

### **2.4.1 Data and Sample**

The data I use are the Quarterly Census of Employment and Wages (QCEW), which are derived from the ES-202 program. State Workforce Agencies provide information on total pay and employment for covered workers in the state and federal Unemployment Insurance Program. These summaries result from the administration of state unemployment insurance programs. In light of this, the coverage of the QCEW is extensive, representing 98% of covered employment in the United States. The Bureau of Labor Statistics publishes the quarterly data at the county, CBSA, state, and national level. Before 2002, not all states and counties were covered in the QCEW reporting. Therefore, I restrict the data to the years 2002 to 2006, which was a period of active living-age adoption in cities across the United States.

As mentioned earlier, there is no publicly available data that has the city as the unit of observation. This has proven problematic for analysis of living wages on a national scale. Previous research has used the Metropolitan Statistical Area designation in, for example, CPS Outgoing Rotation Group data. MSAs usually contain within them not only the living wage city and its county, but many neighboring counties as well. Using QCEW county data is an improvement in terms of geographic precision and its nearly universal coverage of the U.S. labor market. However, the information contained within the data is limited simply to total number of establishments, number of employees, and wages by county and industry. The QCEW reports total employment for each month in the quarter, based on the number of employees who worked during a pay period that includes the 12th of the month. Total wages are calculated using the reported wages for each month and averaging over the three months in the quarter. The QCEW from 2002

to 2006 reports total employment and total wages for each industry using the 2002 North American Industry Classification System (NAICS).

The outcomes of interest for this study are the log of average wages for a given quarter, calculated by dividing total wages by total employment; total number of establishments, also logged; and log total employment. The QCEW aggregates over all workers; thus it is impossible to distinguish between part-time or full-time workers or to calculate an hourly wage. Averages and totals are calculated for each industry separately.

NAICS coding is hierarchical: each level of the system provides an aggregation of information from the next-lowest level. Twenty sectors (identified with a two-digit code) are broken out into subsectors (a three-digit code), which are further broken out into industry groups. The public-use QCEW suffers from data suppression as industry designations become more specific. When fewer than three establishments within a county fit within a specific industry designation, the information is coded to the next-highest level in the hierarchy. To allow a balance between specificity and lack of suppression, I use the three-digit subsector categories in this analysis. From the QCEW, I create a panel of county-by-quarter observations for six industry subsectors that are most likely to be affected by living wages. These include Construction of Buildings (NAICS 236), General Merchandise Stores (NAICS 452), Administrative and Support Services (NAICS 561), Waste Management and Remediation Services (NAICS 562), Accommodations (NAICS 721), and Food Services and Drinking Places (NAICS 722).

Of these subsectors that are potentially covered by living wage policies, Administrative and Support Services are the most likely to work under city contracts and to have wages low enough that living wages “bite.” Some industries within Administrative and Support Services are janitorial, landscaping, business support services, and facilities support. Waste Management and Remediation also is an industry likely covered by city contract. However, as shown in the descriptive statistics, average wages in this industry



might be higher than living wages. Finally, Construction of Buildings includes industrial and municipal builders, who likely contract with cities.

Food Services includes food service contractors, which likely contract with a city for services in, for example, a municipal airport. The subsector also includes chain and limited-service restaurants that may receive subsidies or tax breaks from a living-wage city that include such provisions in its ordinance. General Merchandise Stores are likely only covered by the subsidy or tax-break provisions of a subset of living-wage policies.

To the data, I added information on the federal minimum wage, state minimum wages, and living wages for cities and counties. When a city has passed a living wage ordinance within the time period in question, I assign the living wage to the city's county. Several cities (for example, Philadelphia) are also a county, making the assignment of the living wage geographically precise. I include information for 39 counties and cities that had a living wage in the applicable years, and apply city living wages to their counties. Table 2.1 lists the cities and counties that had a living wage between 2002 and 2006.<sup>2</sup> Living wage information was first compiled using a web and newspaper search. Once a living wage city was identified, I constructed a wage history using the ordinance in question and any updates in wages that had been approved by the city. When in doubt, I confirmed information by calling or e-mailing city or county officials. Since the living wage is applied to the county, I hereafter refer to counties with a living-wage city, or counties with a living wage, as "living-wage counties." Table 2.1 lists the counties in the sample, their cities, their date of adoption, and their initial living wage rate.

Counties were coded into pairs by mapping the data in ArcGIS and identifying coun-

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<sup>2</sup>No official compendium of living wage ordinances exists. In 2011, Swarts and Vasi (2011) asked, "Why did these 77 [cities] enact living wage policies?" All sources available between 2008 and 2012, during the time of my research, reported between 50 and 60 cities that reliably turned out to have living wage ordinances (some cities had adopted one, but it had been repealed before going into effect). Some smaller cities are in counties with a larger living-wage city. I assign the living wage applicable to the larger city. I also drop three counties that do not share a border with a non-living-wage county, as described in the text.

Table 2.1: List of living wage counties and associated cities

| County          | City/County                | Date of enactment | Initial rate |
|-----------------|----------------------------|-------------------|--------------|
| Alameda         | Oakland, CA                | March 1998        | 9.54         |
| Albany          | Albany, NY                 | Sept. 2005        | 11.91        |
| Arlington       | Arlington County, VA       | June 2003         | 10.98        |
| Baltimore       | Baltimore, MD              | Dec. 1994         | 8.76         |
| Bexar           | San Antonio, TX            | July 1998         | 8.03         |
| Cook            | Chicago, IL                | July 1998         | 9.05         |
| Cuyahoga        | Cleveland, OH              | June 2000         | 8.20         |
| Dane            | Madison, WI                | March 2004        | 8.43         |
| Denver          | Denver, CO                 | Feb. 2000         | 8.20         |
| Douglas         | Lawrence, KS               | Oct. 2003         | 9.53         |
| Durham          | Durham, NC                 | Jan. 1998         | 9.51         |
| Erie            | Buffalo, NY                | Aug. 1999         | 7.22         |
| Hamilton        | Cincinnati, OH             | Nov. 2002         | 8.79         |
| Hudson          | Jersey City, NJ            | June 1996         | 7.50         |
| Ingham          | Lansing, MI                | Sept. 2003        | 11.50        |
| Lancaster       | Lincoln, NE                | March 2004        | 9.97         |
| Los Angeles     | Los Angeles, CA            | March 1997        | 8.76         |
| Lucas           | Toledo, OH                 | June 2000         | 10.66        |
| Miami-Dade      | Miami, FL                  | April 2001        | 11.83        |
| Monroe          | Bloomington, IN            | March 2005        | 10.00        |
| Monroe          | Rochester, NY              | Jan. 2001         | 9.52         |
| Montgomery      | Montgomery County, MD      | June 2002         | 10.50        |
| Montgomery      | Dayton, OH                 | July 2003         | 10.62        |
| Multnomah       | Portland, OR               | June 1996         | 8.00         |
| New Haven       | New Haven, CT              | April 1997        | 8.61         |
| New York        | New York City              | Sept. 1996        | 9.60         |
| Orange          | Orlando, FL                | Aug. 2003         | 8.50         |
| Philadelphia    | Philadelphia, PA           | May 2005          | 7.73         |
| Polk            | Des Moines, IA             | July 2004         | 9.00         |
| Prince George's | Prince George's County, MA | June 2003         | 10.50        |
| Sacramento      | Sacramento, CA             | Dec. 2003         | 10.50        |
| Santa Barbara   | Santa Barbara, CA          | March 2006        | 14.00        |
| Santa Fe        | Santa Fe, NM               | Feb. 2003         | 8.50         |
| St. Louis       | St. Louis, MO              | Aug. 2000         | 11.63        |
| St. Louis       | Duluth, MN                 | July 1997         | 7.55         |
| Washtenaw       | Ann Arbor, MI              | March 2001        | 10.20        |
| Wayne           | Detroit, MI                | Nov. 1998         | 10.44        |
| Westchester     | Westchester County, NY     | Nov. 2002         | 11.50        |
| Whatcom         | Bellingham, WA             | Nov. 2002         | 11.50        |

ties that share a border with one another. Each living-wage county is paired with each county with which it shares a border. To define an urban county, I overlaid information from the Census Bureau's Urbanized Areas Cartographic Boundary file. Every living-wage city, and every county with a living wage, in the data lies within an urban area. If an adjacent county also contained an urban area of similar magnitude to the urban area containing the living wage city, it was retained. I removed three California counties because the living-wage cities within them do not share a border with a non-living-wage county. These are San Francisco County, San Jose County, and Ventura County.

### **2.4.2 Descriptives**

Table 2.2 shows descriptive statistics for the overall sample, broken out by the industry subsamples used in the analysis. Of note in this table is the great variability in wages for the different industries identified, and how mean wages might be related to whether a living wage “bites” for a given industry. The Waste Management and Remediation subsector has the highest mean wages when averaged across the full sample, followed by the Construction subsector, which itself has the highest average wages in the urban counties. In the urban counties, the average quarterly Construction wage is equivalent to an hourly wage of approximately \$25, which is much higher than the highest living wage rate reported in table 2.1. Similarly, a quarterly wage in Waste Management and Remediation is in the neighborhood of \$24 per hour. Of course, because wages are totaled for all employees, the sector may have employees whose wages before enactment were lower than the living wage.

In contrast, however, quarterly wages for the other subsectors are quite a bit lower. In the urban counties, Administrative and Support Services have quarterly wages equivalent to approximately \$14 per hour; Accommodations see approximately \$11; and Food

Services come in at around \$7.29 (of course, many of the workers in this category will receive tips along with their hourly wage).

Also of note is that wages, establishment numbers, and employee numbers increase significantly as the sample is reduced to urban counties. Since all living wage policies occur in urban areas, this indicates a lack of comparability between urban and non-urban counties.

## 2.5 Results

Tables 2.3 through 2.8 show results of the different specifications, displaying each industry in turn. In each table, the five separate specifications are shown, first for contemporaneous and then lagged models. Each contemporaneous model includes the log of the minimum and the living wage, plus county and time fixed effects. Each lagged model includes log minimum wages and living wages, and 2-quarter lagged minimum and living wages, plus the same controls specified for the contemporaneous models. Standard errors are clustered at the level at which a new control is placed: at the county level for model 1, the region level for model 2, the CBSA for model 3, and the set of counties that make up pairs for each living wage county for models 4 and 5.

With all models, the independent variable of interest is the living wage, although it is necessary to control for minimum wages to make certain any effect is due to the living wage rather than the minimum wage. Two concerns make any interpretation of the minimum wage effect in these models problematic. The first is that minimum wages may not be binding in a given industry. The second is that variation in minimum wages is captured only by those few counties that cross state lines within metro areas in which one county in the metro area has a living wage, or when a county in another state provides a pair for a living wage county.

Table 2.2: Descriptive statistics

|                      | All counties<br><i>Mean</i> | Metro counties<br><i>Mean</i> | County pairs<br><i>Mean</i> | Urban pairs<br><i>Mean</i> |
|----------------------|-----------------------------|-------------------------------|-----------------------------|----------------------------|
| Quarterly wage       | 5851.83<br>(3708.59)        | 6210.54<br>(3636.13)          | 6755.23<br>(3725.21)        | 7307.23<br>(3971.19)       |
| Cons.                | 8037.51<br>(3101.45)        | 8710.86<br>(2991.39)          | 11160.43<br>(3243.57)       | 12403.32<br>(3106.07)      |
| GMS                  | 4315.56<br>(2154.48)        | 4347.19<br>(785.14)           | 4628.12<br>(829.07)         | 4790.47<br>(977.95)        |
| A&SS                 | 5536.4<br>(2449.00)         | 5608.65<br>(2028.38)          | 6333.30<br>(1568.24)        | 6780.91<br>(1685.84)       |
| WM&R                 | 8960.07<br>(3106.74)        | 9139.58<br>(2970.72)          | 10839.27<br>(2858.72)       | 11323.12<br>(3118.92)      |
| Accom.               | 3584.7<br>(2011.86)         | 3639.27<br>(1343.35)          | 4781.22<br>(2858.72)        | 5280.28<br>(1924.26)       |
| FS                   | 2589.96<br>(1038.31)        | 2727.63<br>(601.27)           | 3235.84<br>(754.04)         | 3501.58<br>(799.65)        |
| Total establishments | 64.13<br>(283.13)           | 100.87<br>(364.88)            | 455.54<br>(1070.72)         | 607.63<br>(1284.42)        |
| Cons.                | 81.7<br>(208.59)            | 129.75<br>(264.93)            | 460.00<br>(646.38)          | 608.18<br>(770.70)         |
| GMS                  | 15.49<br>(36.77)            | 23.69<br>(46.54)              | 81.86<br>(123.14)           | 108.45<br>(143.85)         |
| A&SS                 | 129.78<br>(439.01)          | 203.8<br>(554.07)             | 876.94<br>(1341.49)         | 1166.50<br>(1528.98)       |
| WM&R                 | 8.04<br>(17.59)             | 11.26<br>(21.08)              | 35.29<br>(44.18)            | 45.72<br>(49.78)           |
| Accomm.              | 20.57<br>(45.78)            | 30.43<br>(57.56)              | 35.29<br>(44.18)            | 102.70<br>(134.57)         |
| FS                   | 159.99<br>(522.67)          | 267.76<br>(680.78)            | 1182.31<br>(1857.30)        | 1614.23<br>(2186.70)       |
| Total employment     | 1123.97<br>(6014.39)        | 1841.97<br>(7632.94)          | 8945.63<br>(21583.32)       | 11806.96<br>(26046.97)     |
| Cons.                | 527.99<br>(1779.73)         | 873.49<br>(2296.44)           | 3367.61<br>(4616.45)        | 4253.39<br>(5197.41)       |
| GMS                  | 928.08<br>(2665.00)         | 1543.9<br>(3369.18)           | 5458.60<br>(7741.07)        | 6774.42<br>(8692.80)       |
| A&SS                 | 2394.41<br>(10895.28)       | 3874.43<br>(13415.65)         | 19439.26<br>(34359.58)      | 26640.47<br>(41352.86)     |
| WM&R                 | 114.54<br>(401.22)          | 174.98<br>(494.23)            | 693.28<br>(1083.55)         | 926.83<br>(1250.30)        |
| Accomm.              | 593.48<br>(3912.22)         | 978.38<br>(5134.29)           | 693.28<br>(1083.55)         | 5016.49<br>(8895.86)       |
| FS                   | 2792.98<br>(9612.26)        | 4790.79<br>(12513.70)         | 20746.02<br>(32839.00)      | 27230.18<br>(38383.58)     |
| Number of counties   | 3187                        | 1764                          | 210                         | 113                        |

Standard deviations in parentheses.

It should also be noted that, for a few of the industries in question, coefficients do not have the “expected” sign for wages when looking at the parsimonious model. This is likely due to the inclusion of the East South Central Census region, which experienced strong wage and employment growth over the period in question but did not have any state minimum or city living wage laws. (Dube et al., 2007)

### **2.5.1 Wage Effects**

The effect of living wages on wages is significant in the preferred model for three industries: Administrative and Support Services, Accommodations (only in contemporaneous model) and Food Services (only in lagged model). Administrative and Support Services displays more consistent effects, with an elasticity of 0.041 in the contemporaneous model and 0.067 in the lagged. Considering that this subsector is the most likely to be covered by living wages, this is not surprising.

Minimum wages have a statistically significant positive effect on wages in models 4 and 5 for Food Services, as well as for Construction. The elasticities for Food Services are roughly comparable to Dube et al. (2007), who estimate an upper bound of 0.224 (compared with my upper bound of 0.205) and a lower bound of 0.153 (compared with my lower bound of 0.102) using the QCEW data with a smaller set of counties, a longer span of years, and using specifically the restaurant industry. Construction elasticities are quite large, ranging from 0.191 to 0.291. It should also be noted that the estimates for the Construction subsector vary the most from the parsimonious model to model 5, essentially switching from a statistically significant negative elasticity to a statistically significant positive elasticity of similar magnitude.

### **2.5.2 Establishment effects**

In line with prediction, elasticities for number of establishments is negative for Administrative and Support Services, which also sees the most consistent a positive living wage effect. These elasticities are statistically significant in models 4 and 5 for the subsector, with estimated elasticities ranging from about -0.056 to -0.113. In contrast, Food Services and Accommodation do not display statistically significant effects on establishments, although both elasticities are negative.

For minimum wages, elasticities for Food Services are consistently positive, but are not statistically significant. The Construction subsector also has non-statistically significant positive elasticities. It appears as though minimum wages might not have the same effect on establishment numbers as do living wages.

### **2.5.3 Employment effects**

Contrary to expectation, the same subsectors that see positive living wage effects in models 4 and 5 do not see statistically significant negative elasticities for employment in the same models. Negative elasticities appear for Administrative and Support Services in model 4 and model 5, but none is significant. For Food Services, elasticities are positive or negative, and none is statistically significant. Finally, the Accommodations subsector sees statistically significant negative elasticities in the lagged versions of models 4 and 5, but wage elasticities are not significant in these same models. It should also be noted that positive wage effects and negative employment effects are seen in both Administrative and Support Services and Accommodations in model 3 (using the metro-specific time effects).

For minimum wages, elasticities are positive for the Food Services subsector, although not statistically significant, and negative and not significant for the Construction subsec-

tor.

Overall, the results suggest that living wages have had a positive effect on wages in the industry where we would most expect to see an effect, and this industry sees a decrease in the number of establishments in living wage counties compared with non-living wage counties. In this same industry and in the same models, a decrease in employment is not different from 0. This evidence is suggestive of a monopsony model in which employers who begin to see negative profits from the higher cost of labor will exit the industry. Meanwhile, firms that can absorb the cost may take up the excess labor supply.

It is of note that the effects are different for different subsectors. Administrative and Support Services are most likely to feel the “bite” of a living wage, and this is borne out in the wage elasticities. Food Services may also feel the bite, but the industry may not experience the same level of market exit because of a differential ability to absorb the cost (perhaps a greater ability to pass on the cost in the form of prices). Meanwhile, minimum wages do not appear to have a negative effect on number of firms.

It is slightly surprising that several of the subsectors included in the analysis showed little or no effect of living or minimum wages; however, for these subsectors, wages might already be too high to feel the bite of the wage floor. Waste Management and Remediation has, for example, a higher mean wage than any state minimum wage or living wage. It is perhaps not surprising, then, that wage floors do not appear to affect these industries or to tell a conflicting story.

## **2.6 Falsification test**

If the preceding results are to be believed, it should also be true that attributing a false living wage to counties within the urban designation should not lead to observable wage effects. To test this, I treated each non-living-wage county in an urban group as a “lead”





Table 2.4: Effects of minimum wages and living wages on the general merchandise store industry

|                     | Contemporaneous models     |                           |                           | Lagged models               |                           |                           |
|---------------------|----------------------------|---------------------------|---------------------------|-----------------------------|---------------------------|---------------------------|
|                     | Wages                      | Establishments            | Employment                | Wages                       | Establishments            | Employment                |
| <i>Parsimonious</i> |                            |                           |                           |                             |                           |                           |
| Minimum wage        | b/se<br>-0.065*<br>(0.031) | b/se<br>-0.024<br>(0.056) | b/se<br>-0.018<br>(0.063) | b/se<br>-0.073**<br>(0.025) | b/se<br>-0.061<br>(0.042) | b/se<br>-0.037<br>(0.054) |
| Living wage         | -0.003<br>(0.019)          | 0.083*<br>(0.038)         | -0.057<br>(0.036)         | -0.005<br>(0.012)           | 0.099***<br>(0.017)       | -0.041<br>(0.025)         |
| <i>Region*time</i>  |                            |                           |                           |                             |                           |                           |
| Minimum wage        | 0.058<br>(0.069)           | 0.030<br>(0.094)          | 0.054<br>(0.083)          | 0.057<br>(0.072)            | -0.004<br>(0.118)         | 0.038<br>0.064            |
| Living wage         | 0.024<br>(0.030)           | 0.090<br>(0.052)          | -0.052<br>(0.048)         | 0.013<br>(0.010)            | 0.105***<br>(0.017)       | -0.038<br>0.021           |
| <i>Metro*time</i>   |                            |                           |                           |                             |                           |                           |
| Minimum wage        | -0.038<br>(0.038)          | -0.150*<br>(0.071)        | -0.011<br>(0.081)         | -0.042<br>0.031             | -0.185**<br>(0.065)       | -0.020<br>(0.068)         |
| Living wage         | 0.015<br>(0.026)           | 0.047<br>(0.034)          | -0.059<br>(0.037)         | 0.013<br>0.008              | 0.065***<br>(0.015)       | -0.043*<br>(0.018)        |
| <i>Pairs*time</i>   |                            |                           |                           |                             |                           |                           |
| Minimum wage        | -0.037<br>(0.032)          | 0.023<br>(0.061)          | 0.259*<br>(0.116)         | -0.012<br>(0.041)           | 0.020<br>(0.049)          | 0.233*<br>(0.106)         |
| Living wage         | 0.003<br>(0.021)           | 0.040<br>(0.052)          | -0.090*<br>(0.039)        | 0.001<br>(0.023)            | 0.058<br>(0.023)          | -0.084*<br>(0.031)        |
| <i>Urban*time</i>   |                            |                           |                           |                             |                           |                           |
| Minimum wage        | 0.029<br>(0.044)           | 0.074<br>(0.081)          | 0.220<br>(0.135)          | 0.061<br>(0.044)            | 0.086<br>(0.086)          | 0.252<br>(0.122)          |
| Living wage         | 0.022<br>(0.024)           | 0.025<br>(0.050)          | -0.051<br>(0.038)         | 0.021<br>(0.026)            | 0.025<br>(0.028)          | -0.053<br>(0.039)         |

See footnote for Table 2.3.

Table 2.5: Effects of minimum wages and living wages on the administrative and support services industry

|                     | Contemporaneous models |                      |                      | Lagged models        |                      |                      |
|---------------------|------------------------|----------------------|----------------------|----------------------|----------------------|----------------------|
|                     | Wages                  | Establishments       | Employment           | Wages                | Establishments       | Employment           |
| <i>Parsimonious</i> | b/se                   | b/se                 | b/se                 | b/se                 | b/se                 | b/se                 |
| Minimum wage        | -0.091<br>(0.049)      | 0.033<br>(0.047)     | 0.006<br>(0.099)     | -0.056<br>(0.049)    | 0.028<br>(0.046)     | -0.055<br>(0.095)    |
| Living wage         | 0.019<br>(0.018)       | -0.081***<br>(0.020) | -0.163***<br>(0.036) | 0.018<br>(0.023)     | -0.081***<br>(0.015) | -0.156***<br>(0.037) |
| <i>Region*time</i>  |                        |                      |                      |                      |                      |                      |
| Minimum wage        | 0.079<br>(0.089)       | 0.138<br>(0.159)     | 0.160<br>(0.092)     | 0.109<br>(0.098)     | 0.128<br>(0.156)     | 0.088<br>(0.075)     |
| Living wage         | 0.055**<br>(0.014)     | -0.060<br>(0.028)    | -0.129*<br>(0.044)   | 0.0594***<br>(0.019) | -0.056***<br>(0.014) | -0.116***<br>(0.032) |
| <i>Metro*time</i>   |                        |                      |                      |                      |                      |                      |
| Minimum wage        | -0.043<br>(0.051)      | 0.103<br>(0.071)     | 0.003<br>(0.113)     | -0.023<br>(0.053)    | 0.098<br>(0.066)     | -0.069<br>(0.106)    |
| Living wage         | 0.035*<br>(0.016)      | -0.079***<br>(0.023) | -0.150***<br>(0.041) | 0.037<br>(0.020)     | -0.073***<br>(0.014) | -0.136***<br>(0.033) |
| <i>Pairs*time</i>   |                        |                      |                      |                      |                      |                      |
| Minimum wage        | 0.071<br>(0.092)       | 0.028<br>(0.047)     | 0.046<br>(0.098)     | 0.056<br>(0.096)     | 0.074<br>(0.042)     | 0.141<br>(0.099)     |
| Living wage         | 0.035<br>(0.023)       | -0.098***<br>(0.024) | -0.048<br>(0.039)    | 0.030<br>(0.024)     | -0.113***<br>(0.018) | -0.049<br>(0.035)    |
| <i>Urban*time</i>   |                        |                      |                      |                      |                      |                      |
| Minimum wage        | 0.224<br>(0.124)       | -0.014<br>(0.044)    | 0.066<br>(0.092)     | 0.224<br>(0.126)     | -0.015<br>(0.047)    | 0.086<br>(0.088)     |
| Living wage         | 0.041*<br>(0.018)      | -0.056***<br>(0.016) | -0.026<br>(0.046)    | 0.067*<br>(0.031)    | -0.058***<br>(0.011) | (-0.013)<br>(0.029)  |

See footnote for Table 2.3.

Table 2.6: Effects of minimum wages and living wages on the waste management and remediation services industry

|                     | Contemporaneous models |                   | Lagged models     |                    |
|---------------------|------------------------|-------------------|-------------------|--------------------|
|                     | Wages                  | Employment        | Wages             | Employment         |
|                     | b/se                   | b/se              | b/se              | b/se               |
| <i>Parsimonious</i> |                        |                   |                   |                    |
| Minimum wage        | -0.090<br>(0.050)      | 0.022<br>(0.088)  | -0.209<br>(0.137) | -0.187<br>(0.119)  |
| Living wage         | -0.042<br>(0.029)      | -0.063<br>(0.060) | 0.006<br>(0.075)  | 0.038<br>(0.033)   |
| <i>Region*time</i>  |                        |                   |                   |                    |
| Minimum wage        | 0.081<br>(0.054)       | 0.089<br>(0.264)  | -0.051<br>(0.087) | -0.024<br>(0.079)  |
| Living wage         | -0.003<br>(0.048)      | -0.052<br>(0.074) | 0.045<br>(0.083)  | 0.070**<br>(0.020) |
| <i>Metro*time</i>   |                        |                   |                   |                    |
| Minimum wage        | -0.025<br>(0.062)      | -0.025<br>(0.109) | -0.174<br>(0.166) | -0.148<br>(0.137)  |
| Living wage         | -0.025<br>(0.036)      | -0.089<br>(0.060) | 0.024<br>(0.077)  | 0.048<br>(0.030)   |
| <i>Pairs*time</i>   |                        |                   |                   |                    |
| Minimum wage        | 0.067<br>(0.074)       | 0.161<br>(0.139)  | 0.021<br>(0.082)  | -0.314<br>(0.164)  |
| Living wage         | 0.006<br>(0.035)       | -0.075<br>(0.073) | 0.017<br>(0.067)  | 0.046<br>(0.038)   |
| <i>Urban*time</i>   |                        |                   |                   |                    |
| Minimum wage        | 0.191<br>(0.129)       | 0.165<br>(0.102)  | -0.117<br>(0.256) | 0.001<br>(0.245)   |
| Living wage         | 0.001<br>(0.034)       | -0.097<br>(0.088) | 0.010<br>(0.077)  | -0.051<br>(0.080)  |

See footnote for Table 2.3.

Table 2.7: Effects of minimum wages and living wages on the accommodations industry

|                     | Contemporaneous models |                   |                     | Lagged models       |                   |                    |
|---------------------|------------------------|-------------------|---------------------|---------------------|-------------------|--------------------|
|                     | Wages                  | Establishments    | Employment          | Wages               | Establishments    | Employment         |
| <i>Parsimonious</i> | b/se                   | b/se              | b/se                | b/se                | b/se              | b/se               |
| Minimum wage        | 0.001<br>(0.036)       | -0.025<br>(0.044) | 0.044<br>(0.073)    | -0.004<br>(0.035)   | -0.032<br>(0.042) | 0.049<br>(0.073)   |
| Living wage         | 0.012<br>(0.018)       | 0.005<br>(0.018)  | -0.063*<br>(0.025)  | 0.016<br>(0.018)    | 0.004<br>(0.015)  | -0.067*<br>(0.032) |
| <i>Region*time</i>  |                        |                   |                     |                     |                   |                    |
| Minimum wage        | 0.130<br>(0.059)       | -0.002<br>(0.100) | 0.062<br>(0.056)    | 0.121<br>(0.064)    | -0.010<br>(0.096) | 0.063<br>(0.074)   |
| Living wage         | 0.042*<br>(0.018)      | 0.008<br>(0.021)  | -0.064**<br>(0.018) | 0.046***<br>(0.012) | 0.007<br>(0.011)  | -0.068*<br>(0.031) |
| <i>Metro*time</i>   |                        |                   |                     |                     |                   |                    |
| Minimum wage        | 0.056<br>(0.036)       | -0.025<br>(0.051) | 0.066<br>(0.085)    | 0.056<br>(0.035)    | -0.047<br>(0.049) | 0.048<br>(0.084)   |
| Living wage         | 0.034<br>(0.018)       | -0.014<br>(0.020) | -0.057*<br>(0.026)  | 0.034*<br>(0.017)   | -0.015<br>(0.014) | -0.064*<br>(0.032) |
| <i>Pairs*time</i>   |                        |                   |                     |                     |                   |                    |
| Minimum wage        | -0.017<br>(0.088)      | 0.060<br>(0.064)  | 0.162<br>(0.088)    | 0.012<br>(0.090)    | 0.110<br>(0.060)  | 0.169<br>(0.090)   |
| Living wage         | 0.035<br>(0.024)       | 0.003<br>(0.025)  | -0.051<br>(0.034)   | 0.011<br>(0.028)    | -0.006<br>(0.027) | -0.070*<br>(0.032) |
| <i>Urban*time</i>   |                        |                   |                     |                     |                   |                    |
| Minimum wage        | 0.074<br>(0.088)       | 0.071<br>(0.044)  | 0.088<br>(0.074)    | 0.098<br>(0.076)    | 0.068<br>(0.046)  | 0.071<br>(0.062)   |
| Living wage         | 0.050*<br>(0.022)      | -0.027<br>(0.032) | -0.049<br>(0.033)   | 0.016<br>(0.024)    | -0.030<br>(0.029) | -0.081*<br>(0.035) |

See footnote for Table 2.3.

Table 2.8: Effects of minimum wages and living wages on the food services industry

|                     | Contemporaneous models |                     |                    | Lagged models       |                     |                   |
|---------------------|------------------------|---------------------|--------------------|---------------------|---------------------|-------------------|
|                     | Wages                  | Establishments      | Employment         | Wages               | Establishments      | Employment        |
| <i>Parsimonious</i> | b/se                   | b/se                | b/se               | b/se                | b/se                | b/se              |
| Minimum wage        | 0.019<br>(0.020)       | 0.096**<br>(0.031)  | -0.086*<br>(0.039) | 0.009<br>(0.019)    | 0.095***<br>(0.030) | -0.071<br>(0.007) |
| Living wage         | 0.011<br>(0.010)       | 0.022<br>(0.013)    | 0.005<br>(0.014)   | 0.012<br>(0.008)    | 0.017<br>(0.010)    | 0.039<br>(0.014)  |
| <i>Region*time</i>  |                        |                     |                    |                     |                     |                   |
| Minimum wage        | 0.135*<br>(0.057)      | 0.136<br>(0.120)    | -0.038<br>(0.081)  | 0.126**<br>(0.059)  | 0.137<br>(0.125)    | -0.019<br>(0.086) |
| Living wage         | 0.039<br>(0.022)       | 0.034*<br>(0.013)   | 0.018<br>(0.012)   | 0.041***<br>(0.007) | 0.029***<br>(0.009) | 0.019<br>(0.012)  |
| <i>Metro*time</i>   |                        |                     |                    |                     |                     |                   |
| Minimum wage        | 0.075*<br>(0.031)      | 0.138***<br>(0.040) | 0.023<br>(0.037)   | 0.062*<br>(0.027)   | 0.149***<br>(0.041) | 0.037<br>(0.039)  |
| Living wage         | 0.021<br>(0.014)       | -0.000<br>(0.014)   | -0.009<br>(0.011)  | 0.024**<br>(0.009)  | -0.004<br>(0.010)   | -0.005<br>(0.015) |
| <i>Pairs*time</i>   |                        |                     |                    |                     |                     |                   |
| Minimum wage        | 0.102***<br>(0.027)    | 0.035<br>(0.046)    | 0.050<br>(0.048)   | 0.104***<br>(0.027) | 0.012<br>(0.046)    | 0.053<br>(0.045)  |
| Living wage         | 0.001<br>(0.009)       | 0.002<br>(0.019)    | 0.002<br>(0.019)   | 0.004<br>(0.011)    | 0.004<br>(0.008)    | -0.005<br>(0.014) |
| <i>Urban*time</i>   |                        |                     |                    |                     |                     |                   |
| Minimum wage        | 0.205**<br>(0.064)     | 0.141<br>(0.107)    | 0.129<br>(0.098)   | 0.196***<br>(0.056) | 0.126<br>(0.109)    | 0.101<br>(0.102)  |
| Living wage         | 0.008<br>(0.009)       | -0.025<br>(0.016)   | -0.000<br>(0.021)  | 0.018*<br>(0.007)   | -0.022<br>(0.014)   | 0.001<br>(0.019)  |

See footnote for Table 2.3.

county for the pairs, and then paired each target county in turn with all other counties in the group. I then randomly assigned the living wage applicable for the urban group to the pairs. I used time-specific group fixed effects to see whether the false living wage led to a wage effect.

### 2.6.1 False living wage

Table 2.9 shows the results of the falsification test in which the fictitious living wage was applied to non-living-wage counties within the urban group that makes up each urban pair. A model was run for each using time-specific group fixed effects, and clustered at the group level. Subsectors include Administrative and Support Services and Food Services, which were the two subsectors that showed evidence of a positive wage effect.

Table 2.9: Effects of fictitious living wage

|                   | Subsector        |                  |                  |                   |
|-------------------|------------------|------------------|------------------|-------------------|
|                   | A&SS             |                  | FS               |                   |
|                   | Contemp.         | Lagged           | Contemp.         | Lagged            |
| False living wage | 0.004<br>(0.005) | 0.014<br>(0.014) | 0.001<br>(0.001) | -0.003<br>(0.005) |

Neither industry shows a wage effect that is statistically significant, and for Food Service, the lagged estimate, which was positive and statistically significant for the “true” living wage, is here negative and not significant. The test gives evidence that the effect being picked up for the true value of the living wage is reliable.

## 2.7 Conclusion

The findings in this paper are roughly in line with those found by Lester (2011) in the most recent work available on this question. It also corresponds with earlier case studies

on individual cities, such as those performed by Fairris (2005) and Reich et al. (1999). In each case, it was found that living wage ordinances do not have large negative impacts on employment, contrary to theory. The elasticities on wages that I find are roughly similar to those found in these studies, as well as in the work of Neumark and coauthors. In my preferred specifications using urban county pairs, I also do not find a negative impact on the number of establishments in living wage cities, or negative impacts on establishments due to minimum wages. This constitutes one of the few tests of Rebitzer and Taylor (1995) in the literature.

There are a number of innovations in this study in regards to investigating living wages. It is the first study to look at living wages at a national level and use the county as the geographic identifier rather than the MSA (or CBSA). Thus the application of living wages to a geographic area is more precise than in earlier national studies. Second, it employs an empirical strategy that controls for spatial heterogeneity and both serial and temporal autocorrelation in order to disentangle living wage adoption from its effects in local labor markets.

There are also several limitations to this study. As with all national studies of the living wage, in the absence of city-based data the geographic application of living wages will be imprecise. Moreover, this study similarly suffers from lack of good information on coverage of living wages in relation to industries. There are also issues specific to this study. By using a data set that has limited information—containing only establishment numbers, total employment, and total wages—there may be remaining differences between cities that are not adequately controlled for using fixed effects. To the extent that features of living wage cities, such as union density and income inequality, influence the adoption of living wage ordinances, it would definitely be an improvement to have city-based measures of these characteristics. A good next step might be to use city characteristics to match living-wage cities with non-living wage cities that have similar attributes (not



necessarily sharing a county border). Chapter 3 presents a study based on such a design, albeit one with its own limitations.

## CHAPTER 3

### THE EFFECT OF LIVING WAGES ON WAGES, EMPLOYMENT, AND HOURS WORKED: NEW EVIDENCE USING PROPENSITY SCORE MATCHING

#### 3.1 Introduction

In 2007, the U.S. Congress raised the federal minimum wage for the first time in 10 years. In the preceding decades, in response to the declining real value of the minimum wage, several states set their own wage floors or passed laws that extended the coverage of the minimum wage (Card (1992)). Many cities, as well, began setting wage floors that covered certain categories of employers and workers. Beginning in December 1994, with passage of an ordinance Baltimore, Md. (Neumark and Adams (2003)), more than 40 cities around the United States have enacted living wage policies, along with several counties and college campuses ([livingwagecampaign.org](http://livingwagecampaign.org), last accessed Oct. 27, 2009).

Policymakers who argued against raising the federal minimum wage for so long successfully deployed their main argument: that increasing the minimum wage would decrease the demand for labor, causing higher unemployment that would disproportionately affect those at the lower end of the wage distribution. According to this argument, mandated higher wages will cause employers to move toward cutting jobs and/or hours, as well as move to higher-skilled workers at the expense of lower-skilled workers.

The jury is still out, however, on whether this phenomenon of decreased employment at the lower end of the pay distribution actually occurs. While some studies have found that increased minimum wages have no effect on employment, or even have a positive effect (Card (1992); Card and Krueger (1994)), others have found the opposite (Neumark and Wascher (1992); Neumark and Wascher (2000); Burkhauser et al. (2000)). The question is an important one: a worker's wages are tied to the ability to support a family, and work

and wages are fundamental to issues of overall income equality and equity in society. Instinct tells us that people should be paid wages they can live on. On the other hand, if these wage floors lead to lower employment rates and increased joblessness, they will have failed in their attempt to improve the lot of low-wage workers.

While the effect of a minimum wage has been extensively studied, living wages have not had the same attention. Living wage mandates present an analytical challenge in that they are defined differently from city to city; within a given city, they are unevenly applied to workers; and cities that adopt living wages tend to look different from other cities in ways that likely affect both living wage adoption and subsequent effects (for example, a living wage city might adopt an ordinance because its median wage is comparatively high). Besides the analytical challenge presented, there is also no reliable depository of information on living wages, nor is there a data set that captures important features of workers that would identify them as being covered by an ordinance.

This paper attempts to pick up the effects of living wage ordinances on wages and subsequent employment using Current Population Survey data from 1996 to 2000. In light of the analytical challenges presented in the preceding paragraph, I limit my analysis to the same years analyzed by Neumark and Adams (2003). I do this for two other reasons: the first is that I wish to investigate whether their results are sensitive to certain sample restrictions that I place on the data; and information on living wage rates after 2000 would need to be compiled city-by-city—a labor-intensive process that will need to be undertaken for later versions of this paper.

The work presented here contributes to the discussion by better identifying the control group to which living-wage cities are compared. I use a propensity score matching procedure that generates a comparison group of individuals in cities that never adopt a living wage ordinance. This comparison group more closely resembles individuals in living wage cities on key characteristics of both the individuals and the cities in which

they reside. Then, using matched data, I use a difference-in-differences set-up to compare changes over time in living wage cities compared with non-living-wage cities. The parametric analysis draws on Neumark and Adams' (2003) evaluation of the effects of living wages (hereinafter referred to as NA). I use the same data, including their compilation of wage floors for both states and cities, and adopt similar strategies in terms of estimation and sample restrictions. However, the matching that precedes analysis results in a different sample, one that more closely resembles the gold standard of random assignment to a treatment and control group. I hypothesize that, upon using a comparable control group as the counterfactual, differences in employment characteristics between living-wage and non-living-wage cities will not longer be significant.

## **3.2 Background**

### **3.2.1 Living Wage Policy**

Living wages are similar to minimum wages in the sense that they set an absolute minimum that employers must pay. However, in terms of enforcement, living wage laws generally only cover public employees or employees of businesses that contract with the city or otherwise acquire public money. In this sense they are related to "prevailing wage" requirements (Martin, 2001). Many municipalities across the United States have instituted living wage ordinances, and these tend to vary greatly. While some cities and towns have simply required that city/town employees be paid a living wage (set at an hourly rate that will cover basic needs in the municipality in question), others have much more far-reaching rules. Businesses contracting with the municipality and businesses receiving subsidies from the municipality are often included in the ordinance. Because certain large employers often receive substantial tax breaks and other subsidies ("big box" stores often

fit in this category), these ordinances have raised the wages of workers at such stores well beyond the federal minimum wage ([livingwagecampaign.org](http://livingwagecampaign.org)<sup>1</sup>).

Living wages vary dramatically in their levels, as well. Table 3.1 shows the average living wage rate for the years under analysis in this chapter, as well as the median wage for each year using the entire CPS sample of workers in SMSAs. While it is often the case that cities with a median wage above the average for living wage cities set higher wage floors, this is not always the case. For example, San Antonio's living wage is among the highest among living wage cities, yet its median wage is below average for both living-wage and non-living-wage cities. As shown in the table, however, living-wage cities consistently experience higher median wages than do non-living-wage cities.

Because of their uneven application, living wages may influence changes in wages or employment through a channel distinct from the usual economic effect of a wage floor. Living wages in one sector of the workforce may cause a shift outward in the supply curve of labor for those of similar skills in the uncovered sector, as employment in the covered sector shrinks (NA). This could, perversely, cause wages to change downward overall, if the shift down in wages in the uncovered sector offsets the rise in the covered sector. We would at any rate expect that the effect of living wages on average wages will be less dramatic than the effects of a minimum wage.

### **3.2.2 Previous research**

Most of the important empirical work on living wages has been conducted by David Neumark and colleagues (Neumark (2002); Neumark and Adams (2003); Adams and Neumark (2005b); Adams and Neumark (2005c). His work has consistently found a slight

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<sup>1</sup>The web site [livingwage.org](http://livingwage.org) was an extremely useful compilation of living wage campaigns, successful and ongoing, around the U.S. The organization and the web page were connected to the group ACORN, and the information disappeared after the group's demise in 2010. I captured and printed off the pages in question on October 29, 2009.

Table 3.1: Living wages in the U.S., 1996-2000

| City            | 1996        |             | 1997        |             | 1998        |             | 1999        |             | 2000        |             |
|-----------------|-------------|-------------|-------------|-------------|-------------|-------------|-------------|-------------|-------------|-------------|
|                 | Living wage | Median wage | Living wage | Median wage | Living wage | Median wage | Living wage | Median wage | Living wage | Median wage |
| Baltimore       | 6.38        | 15.50       | 6.85        | 15.92       | 7.39        | 16.57       | 7.80        | 17.14       | 7.90        | 16.75       |
| Boston          |             |             |             |             | 8.23        | 17.38       | 8.29        | 18.23       | 8.44        | 19.15       |
| Buffalo         |             |             |             |             |             |             |             |             | 6.22        | 14.78       |
| Chicago         |             |             |             |             | 7.60        | 16.09       | 7.60        | 16.68       | 7.60        | 17.45       |
| Dayton-Spr.     |             |             |             |             | 7.00        | 13.28       | 7.00        | 13.92       | 7.00        | 14.93       |
| Denver          |             |             |             |             |             |             |             |             | 8.20        | 17.42       |
| Detroit         |             |             |             |             | 8.23        | 17.08       | 8.33        | 16.74       | 8.50        | 17.74       |
| Hartford        |             |             |             |             |             |             | 9.19        | 19.09       | 9.34        | 18.41       |
| Jersey City     | 7.50        | 13.10       | 7.50        | 11.26       | 7.50        | 12.69       | 7.50        | 13.65       | 7.50        | 14.27       |
| LA-Long Beach   |             |             | 7.25        | 13.51       | 7.32        | 14.55       | 7.44        | 15.15       | 7.60        | 15.52       |
| Milwaukee       | 6.21        | 13.77       | 6.38        | 14.49       | 6.54        | 14.24       | 6.65        | 15.12       | 6.78        | 17.00       |
| Minneapolis     |             |             | 8.03        | 15.15       | 8.20        | 16.41       | 8.33        | 17.80       | 8.50        | 18.86       |
| Oakland         |             |             |             |             | 8.00        | 19.57       | 8.11        | 20.95       | 8.30        | 21.42       |
| Omaha           |             |             |             |             |             |             |             |             | 8.19        | 15.53       |
| Portland        | 7.00        | 13.73       | 7.00        | 14.01       | 7.26        | 15.06       | 7.77        | 16.00       | 8.00        | 16.52       |
| Raleigh         |             |             |             |             | 7.55        | 15.83       | 7.55        | 16.28       | 7.55        | 17.50       |
| St. Louis       |             |             |             |             |             |             |             |             | 8.84        | 15.00       |
| San Antonio     |             |             |             |             | 9.27        | 11.67       | 9.27        | 11.75       | 9.27        | 12.35       |
| San Francisco   |             |             |             |             |             |             |             |             | 9.00        | 20.80       |
| San Jose        |             |             |             |             | 9.50        | 21.27       | 9.65        | 20.68       | 9.87        | 22.39       |
| Tuscon          |             |             |             |             |             |             | 8.00        | 10.81       | 8.00        | 12.50       |
| Av. lw city     | 6.77        | 14.03       | 7.17        | 14.06       | 7.83        | 15.84       | 8.03        | 16.25       | 8.12        | 16.97       |
| Av. non-lw city |             | 13.47       |             | 13.91       |             | 14.56       |             | 15.40       |             | 15.98       |

negative effect on employment in cities with living wage ordinances compared to those without. In a large study produced for the Public Policy Institute of California, he examined living wage ordinances in that state and assessed the overall impact of these ordinances on low-wage earners and their families. He found that living wage ordinances in California had some positive effects for low-wage earners, including increased wages, but also had a negative effect in the form of decreased employment among those same workers (Neumark (2002)). In a study critical of Neumark's work, Pollin et al. discuss the limitations of this particular study, including Neumark's focus on only low-wage earners and his oversampling of workers in Los Angeles County (Pollin et al. (2002b)). In the study that I base my reanalysis on from 2003, NA found a modest decrease in poverty and a moderate negative employment effect in municipalities with living wage ordinances. In this case, the researchers looked at municipalities across the country. Finally, in an update to the 2003 analysis, and also using MORG data, Adams and Neumark found results similar to their earlier study (Adams and Neumark (2005c)).

Several other studies have been produced that evaluate the employment effect of living wage laws, although several of these were predictive studies rather than *post facto*, or they examine only at a single city. Schoenberger (2000) found no evidence that a living wage ordinance in Baltimore increased the costs of contracts to the city. Pollin et al. (2002a), in a pre-ordinance survey of New Orleans-based businesses, determined that the cost of a living wage could be absorbed by businesses in the form of price and productivity changes and wage redistribution within the businesses; however, it should be noted that this study was projective in nature and not empirical. In a study published in the U.K., Buss and Franceschi (2003) examined 40 U.S. cities with living wage ordinances, finding that 30 cities experienced positive employment effects relative to the broader area surrounding them. Using difference-in-differences estimation, Brenner (2005) found that employers covered by a living wage ordinance in Boston did not decrease employment;

rather, they moved from part-time to full-time work. Brennan also found that these employers experienced wage-compression after the application of the ordinance.

An analysis by Reich et al. (2005) focused on the San Francisco living wage as it applied to workers at the San Francisco airport. The authors found that about 73 percent of the ground-based non-managerial workers at SFO experienced wage increases due to the policy, a cost that was offset by dramatically reduced turnover, improved worker morale, and greater work effort. The authors found no evidence of a decline in employment.

In an analysis of the predictors of a city enacting a living wage ordinance, Gallet (2004) found that city-level annual per capita manufacturing earnings, fair market rent, proximity to another living wage city, and a state minimum wage level above the federal level had significantly positive effects on whether a city adopts a living wage. Other factors, such as characteristics of cities in terms of race and age, did not appear to have an effect. Gallet also controlled for percent voting for Al Gore in 2000 in the county in which the city resides; this was not a significant contributing factor, but the author points out that cities might differ in political preference from the counties they are in. Since there is good reason to believe that a city's political makeup contributes to its propensity to adopt living wages, hopefully this is a feature that can be captured in the future.

### **3.3 Research design**

The research design I employ involves a number of steps. First, an analysis is made of the effects of living wage ordinances on wages. It is necessary to deduce this effect first, as it is not obvious that living wages are implemented in a consistent enough manner to have an effect on wages overall. The design first uses propensity score matching, described below, to generate an analysis subset from the data; then a difference-in-differences approach is used to determine the change in wages experienced by those in living wage cities com-



pared with those in non-living-wage cities. Differences are calculated for four levels of the income distribution, described below in greater detail. Effects of living wages on employment are then investigated using predicted wages and a similar matching procedure. Finally, I look at number of hours worked.

### 3.3.1 Propensity score matching

The matching method used in this analysis is premised on techniques outlined by Rubin (1979) and Rubin (1973), and Rosenbaum and Rubin (1984), but conceptualized under a construct developed by Ho et al. (2007). Ho and his coauthors describe using propensity score matching as a way of “preprocessing” data before estimation of parameters. The goal of preprocessing is to arrive at a sample that mimics one arrived at through random selection. Preprocessed data arrives at a subset of the observed sample for which the treatment and the independent characteristics are unrelated. In other words, the “control group” and “treatment group” are statistically identical on observables. Matching arrives at a sample for which the following holds

$$p(X|T = 1) = p(X|T = 0) \quad (3.1)$$

where  $p$  is the probability density of the data, rather than a population density.

It is an advantage to have large sample when implementing preprocessing, as the most reasonable approach is nearest-neighbor matching. I use the package “psmatch2” in Stata to match nearest neighbors. The procedure boils down to “pruning” observations that are off the common support – that is, individuals in the base “control” group who look substantially different from anyone in the “treatment” group are removed from the sample. This generates a sample that has few restrictions upon it except to remove individuals

who look substantially different from individuals in the treatment group. One effect it does have is to remove anyone from the East South Central region, discussed below.

In general, propensity score matching techniques have been paired with a simple weighted differences in means as the analytical step. Ho et al. suggest that once matching has been implemented, the preprocessed data can then be used in a parametric model to investigate effects. Since the model used here is a difference-in-differences estimation, the subsample that is on the common support is pooled and the analysis performed on the pooled data.

### 3.3.2 Model specification

My difference-in-differences model is similar to that used by NA. In order to make results comparable across our analyses, I also use similar sample restrictions, which are implemented after matching. Since living wages are likely to have an effect at the lower end of the wage distribution, I look at workers at the 10th percentile and below, between the 10th and 25th percentiles, between the 25th and 50th percentiles, and between the 50th and 75th percentiles. Each percentile rank is calculated for a given city-month cell, and then observations are pooled together based on their place in the distribution. The following equation is estimated for each percentile range

$$\ln(w_{ijst}^p) = \alpha + X_{ijst}\omega + \beta * \ln(w_{jst}^{min}) + \gamma * \max[\ln(w_{jst}^{liv}), \ln(w_{jst}^{min})] + \delta_Y Y_t + \delta_M M_s + \delta_C C_j + \epsilon_{ijst} \quad (3.2)$$

where  $w^p$  is the hourly wage for individuals in a given part of the wage distribution;  $X$  is a vector of demographic characteristics including sex, age, race, marital status, and educational attainment;  $w^{min}$  is the higher of the state minimum wage and the federal minimum wage; and  $w^{liv}$  is the higher of the minimum wage and the city living wage.

There is a correlation between states that adopt a minimum wage and cities within them that adopt a living wage. Thus, in order to specifically distinguish the effects of living wages, it is important to control for state minimum wages. It is also important to look at the lagged effects of both variables, as employers in living wage cities may respond to changes in a wage floor with a lag (Neumark and Adams (2003)).

### **3.3.3 Data**

I use CPS Merged Outgoing Rotation Group (ORG) files from 1995 to 2000 for wages, employment, and worker characteristics. These files provide information on the labor market characteristics for individuals in the CPS who are rotating out of the sample. The data include an SMSA identifier for all cities in the U.S., as well as information on whether an individual lives within the central city. Unfortunately, information does not exist that identifies where people work. Considering how large some SMSAs are, and how much suburban territory is contained within them, any study of the living wage will necessarily be hampered in its ability to identify exactly who is covered. It is likely that workers who are employed by firms covered by a living wage, and who are in the wage distribution most likely affected by the wage floor, live in the same SMSA where they work, but this is not guaranteed.

Included in the ORG data is information on hourly wages, industry and occupation, union and veteran status, and an array of personal and family characteristics. These data are used both in propensity score matching, described later, and in the analysis of wages, employment, and hours.

In light of the analysis by Gallet (2004), it seemed necessary to control for city labor market characteristics when matching cities—specifically, employment or business income growth. This information is not available in the MORG data, so I gathered employ-

ment growth information from the Bureau of Labor Statistics County Business Patterns, metropolitan files, from 1995 to 2000. These data are generated on a quarterly basis, and report the numbers of people employed overall in a given SMSA and in individual industries. For the purposes of matching, I use the overall change in employment from quarter to quarter within an SMSA.

Stata code that generates the data on living wage rates was generously provided by Scott Adams. These same rates were used in NA. The code generates living wage rates for SMSAs over the time period. Some cities reported to have living wage mandates over the time period actually have prevailing wage laws. These are not included in the data, nor are the counties and school districts that have adopted living wage ordinances. NA's information was obtained via correspondence with city governments the Employment Policies Institute ([www.epionline.org](http://www.epionline.org)) and the now defunct Association of Community Organizations for Reform Now ([www.acorn.org](http://www.acorn.org), [livingwagecampaign.org](http://livingwagecampaign.org)). NA's confidence in the data is bolstered by its consistency across sources.

All but two laws were passed after 1996 (one was passed in 1994 and the other in 1995). However, the CPS data have a gap in reporting of SMSA codes for 1995, and thus information on SMSA is missing for some months in the data. City-months for 1995 are used for lagging the living wage and minimum wage variables, but data from the year 1995 are not used otherwise in the analysis. Table 3.1 lists the average living wage rate by city and year, and lists the average wage within each city for the period in question.

### **3.3.4 Matching procedure**

The key issue with analyzing the effects of living wage ordinances is the difficulty in determining who is covered. Not only do living wages rates vary dramatically from city to city, but they differ in how they are implemented and which industry and firms are

covered. Studies have also found that ordinances vary in their effectiveness, with some cities doing a better job with implementation than others (Adams and Neumark (2005b)).

Still, even with this variety in ordinances and their implementation, it would still be possible to statistically detect their effects if we were confident that there are no confounding differences between living-wage and non-living-wage cities. Unfortunately, this is not the case. A simple comparison of means in the data indicate that cities that implement a living wage look quite different from cities that do not. Moreover, these differences are likely correlated with job-market outcomes.

In the past, national-level analyses of living wage ordinances have simply compared all living-wage cities to all non-living wage cities using a difference-in-differences approach. These analyses are not well identified, because the differences between cities that lead some to adopt ordinances have differences in characteristics that likely have different pre-treatment and post-treatment trends. A simple example of this is the fact that no city in the Census-defined East South Central region (comprising the states Kentucky, Tennessee, Alabama, and Mississippi) have adopted a living wage (and none of these states had adopted a minimum wage), and yet these states have seen greater economic growth, unrelated to the adoption of wage floors, over the period in question compared with other regions of the country (Dube et al. 2007). Including cities from this region as a control may exaggerate the effect of living wages on employment simply because of unrelated differences in preexisting trends.

I have attempted to overcome the identification issue by using propensity score matching to match individuals in living wage cities with individuals in non-living-wage cities. Thus, the matching procedure asks, “When I have a person with given characteristics, and facing certain characteristics of his or her living wage city, who looks like that person in a non-living-wage city?”

Any difference-in-differences estimate is predicated on the assumption that the control

group and the treatment group experience unvarying trends in the feature under analysis both before and after the treatment. A matching strategy such as the one I employ should control for any preexisting differences in important trends by matching on economic and individual characteristics within regions and using year fixed effects. Thus, individuals in living wage cities are matched to controls both before and after enactment.

The variables used for matching individuals appear in table 3.2. Matching proceeds using year fixed effects; the table reports differences in means by year for an indication of differences and their stability. Clearly, the two types of cities differ in most characteristics (sample sizes are such that nearly all differences in means are statistically significant), and these differences are the most marked in features that may also affect trends in employment and wages. The largest differences between cities are seen in union membership, racial and ethnic makeup, education, and, in earlier years, gender composition. Individuals in living wage cities tend to be slightly younger, more likely to work at a job that pays an hourly wage, and less likely to be a veteran.

Differences are most marked in geographic dispersion of living wage cities. Individuals in living-wage cities are far more likely to live in the Pacific region, East North Central region, or West North Central region. The strength of these differences was such that balance could not be attained by simply adding region as a fixed effect. Thus, propensity score matching was performed for each region, and the matched sample was pooled together.

The occupation variables used in matching are the two-digit Census occupation codes. Considering that living wages tend to target certain occupations and not others, matching individuals of the same occupation in the two groups seemed of particular importance. However, even better would be the ability to compare occupations we know are certainly covered against occupations we know certainly are not. Given the way living wage laws are applied, this is an extremely difficult question to answer, and one that is beyond the

Table 3.2: Sample Characteristics

| means              | Non-living wage cities |        |        |        |        | Living wage cities |        |        |        |        |
|--------------------|------------------------|--------|--------|--------|--------|--------------------|--------|--------|--------|--------|
|                    | 1996                   | 1997   | 1998   | 1999   | 2000   | 1996               | 1997   | 1998   | 1999   | 2000   |
| Age                | 37.611                 | 37.751 | 37.820 | 38.066 | 37.954 | 37.273             | 37.532 | 37.785 | 37.970 | 37.961 |
| Male               | 0.499                  | 0.499  | 0.498  | 0.500  | 0.505  | 0.505              | 0.507  | 0.510  | 0.511  | 0.509  |
| Married            | 0.568                  | 0.561  | 0.551  | 0.548  | 0.541  | 0.556              | 0.552  | 0.539  | 0.549  | 0.539  |
| Black              | 0.107                  | 0.107  | 0.110  | 0.113  | 0.115  | 0.088              | 0.088  | 0.089  | 0.090  | 0.092  |
| Hispanic           | 0.095                  | 0.099  | 0.102  | 0.107  | 0.126  | 0.134              | 0.144  | 0.148  | 0.154  | 0.171  |
| Hourly wage        | 13.468                 | 13.912 | 14.557 | 15.402 | 15.977 | 14.161             | 14.657 | 15.523 | 16.331 | 17.133 |
| Government         | 0.162                  | 0.157  | 0.151  | 0.157  | 0.153  | 0.141              | 0.140  | 0.140  | 0.141  | 0.137  |
| Union              | 0.146                  | 0.144  | 0.138  | 0.136  | 0.136  | 0.170              | 0.169  | 0.167  | 0.167  | 0.163  |
| Veteran            | 0.107                  | 0.101  | 0.096  | 0.096  | 0.090  | 0.091              | 0.090  | 0.088  | 0.084  | 0.081  |
| Paid by hour       | 0.585                  | 0.583  | 0.582  | 0.569  | 0.557  | 0.593              | 0.588  | 0.581  | 0.571  | 0.562  |
| frequencies        |                        |        |        |        |        |                    |        |        |        |        |
| Education          |                        |        |        |        |        |                    |        |        |        |        |
| Less than H.S.     | 0.121                  | 0.121  | 0.124  | 0.120  | 0.123  | 0.124              | 0.135  | 0.132  | 0.130  | 0.136  |
| H.S. grad          | 0.297                  | 0.300  | 0.294  | 0.287  | 0.279  | 0.261              | 0.263  | 0.257  | 0.254  | 0.247  |
| Some college       | 0.289                  | 0.285  | 0.289  | 0.287  | 0.289  | 0.301              | 0.287  | 0.290  | 0.283  | 0.288  |
| College grad       | 0.197                  | 0.199  | 0.198  | 0.205  | 0.208  | 0.207              | 0.212  | 0.217  | 0.223  | 0.221  |
| Advanced degree    | 0.096                  | 0.095  | 0.096  | 0.102  | 0.102  | 0.106              | 0.103  | 0.103  | 0.110  | 0.109  |
| Region             |                        |        |        |        |        |                    |        |        |        |        |
| New England        | 0.041                  | 0.042  | 0.037  | 0.035  | 0.038  | 0.086              | 0.083  | 0.085  | 0.080  | 0.078  |
| Middle Atlantic    | 0.238                  | 0.233  | 0.219  | 0.211  | 0.212  | 0.027              | 0.026  | 0.024  | 0.027  | 0.029  |
| East North Central | 0.094                  | 0.100  | 0.105  | 0.102  | 0.094  | 0.333              | 0.340  | 0.330  | 0.330  | 0.326  |
| West North Central | 0.043                  | 0.045  | 0.048  | 0.052  | 0.057  | 0.125              | 0.124  | 0.127  | 0.123  | 0.125  |
| South Atlantic     | 0.211                  | 0.207  | 0.218  | 0.220  | 0.218  | 0.067              | 0.059  | 0.052  | 0.054  | 0.053  |
| East South Central | 0.028                  | 0.026  | 0.028  | 0.028  | 0.023  | 0.000              | 0.000  | 0.000  | 0.000  | 0.000  |
| West South Central | 0.112                  | 0.113  | 0.111  | 0.114  | 0.114  | 0.023              | 0.019  | 0.022  | 0.024  | 0.022  |
| Mountain           | 0.123                  | 0.123  | 0.124  | 0.129  | 0.134  | 0.056              | 0.060  | 0.063  | 0.061  | 0.056  |
| Pacific            | 0.110                  | 0.111  | 0.111  | 0.109  | 0.111  | 0.283              | 0.290  | 0.298  | 0.301  | 0.312  |
| Number of obs      | 40066                  | 41344  | 40886  | 39063  | 38351  | 18951              | 19566  | 19593  | 19043  | 18125  |

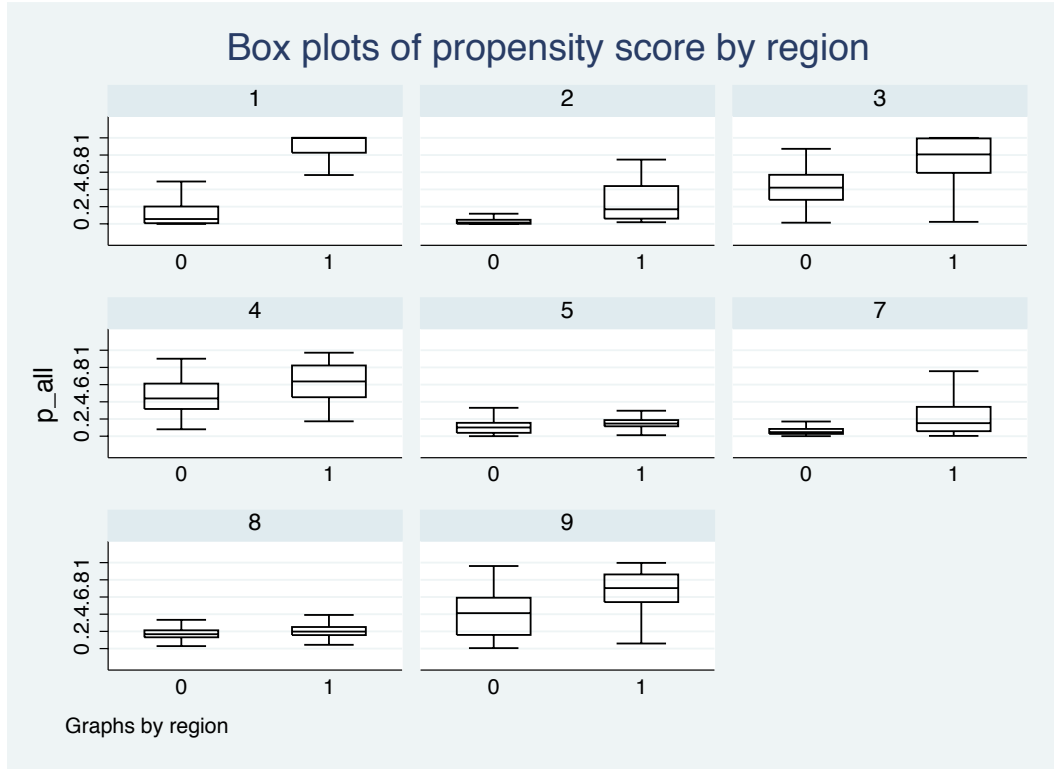
scope of this paper. Future research in this direction would be extremely valuable. For now, it is likely helpful to at least be comparing people of the same occupation between living-wage and non-living-wage cities.

To generate the propensity score, a logit model was used, with the dependent variable being a binary variable that takes on the value of 1 if an individual resides in a city that implements a living wage. The independent variables include age, race, gender, Hispanic origin, marital status, education, union status, and veteran status. I also controlled for whether or not individuals worked by the hour or for the government and their occupation. I also included values for the log of the state or federal minimum wage and its square, as well as employment growth by quarter. Finally, a set of year fixed effects was included. The equation was run for each census-defined region. I then predicted the likelihood that an individual resides in a living-wage city based on the characteristics in question. Nearest neighbor matching was employed to match individuals within living-wage and non-living-wage cities based on their propensity. Since the East South Central region contains no living-wage city, no propensity score was calculated for individuals residing in the region, and the entire region was dropped from the analysis.

Figure 3.1 shows graphically the results of matching using a box-plot for each remaining region. Table 3.3 reports on the sample sizes before and after propensity score matching. In every region, there is a substantial common support. The Eastern U.S. appears to experience greater differences between workers than other regions do. New England is the only region where individuals in living-wage and non-living-wage cities are so different from one another that a large proportion of both living-wage and non-living wage observations are dropped. The Middle Atlantic region has more than half of non-living-wage residents getting dropped from the sample, and the East North Central region loses about a third from the living-wage sample. Other regions to have a greater common sup-



Figure 3.1: Boxplots showing common support by region.



port<sup>2</sup>.

### 3.3.5 Other sample restrictions

I adopted as closely as possible the other sample restrictions used by NA. Like them, I deleted individuals for whom employment information is allocated in the ORG data<sup>3</sup> I also restricted the analysis to workers with an hourly wage greater than one dollar and less than or equal to 100 dollars, and individuals older than 16 and younger than 70.

<sup>2</sup>New England consists of: Maine, New Hampshire, Vermont, Massachusetts, Rhode Island, and Connecticut; living wage cities are Boston and Hartford. The Middle Atlantic region is: New York, New Jersey, and Pennsylvania; living wage cities are Buffalo and Jersey City. The East North Central region is Ohio, Indiana, Illinois, Michigan, and Wisconsin; living wage cities are Detroit, Dayton-Springfield, Chicago, and Milwaukee.

<sup>3</sup>In the ORG, many people report hours worked and salary wage. As in NA, I calculate hourly wages by dividing weekly salary by number of hours worked.

Table 3.3: Sample generated from propensity score matching

| Region             | Sample in non-living-wage city |                | Sample in living-wage city |                |
|--------------------|--------------------------------|----------------|----------------------------|----------------|
|                    | before matching                | after matching | before matching            | after matching |
| New England        | 6394                           | 4932           | 7468                       | 2451           |
| Middle Atlantic    | 44400                          | 20810          | 2506                       | 2506           |
| East North Central | 19740                          | 19735          | 31531                      | 19641          |
| West North Central | 9744                           | 9411           | 11869                      | 10576          |
| South Atlantic     | 42784                          | 41370          | 5417                       | 5416           |
| East South Central | 22462                          | 22161          | 2105                       | 2105           |
| West South Central | 25220                          | 25136          | 5614                       | 5613           |
| Mountain           | 22001                          | 21172          | 28210                      | 24485          |
| Total              | 192745                         | 164727         | 94720                      | 72793          |

Analysis proceeds on percentiles of the wage distribution. These were calculated for each city in each month in the sample; then individuals in each percentile range were pooled together. Again following NA, I drop any cities that do not have at least 25 observations for every month of the 5 years in question.

## 3.4 Results

### 3.4.1 Wage effects

The expectation is that the institution of a wage floor will increase wages at the lower end of the wage distribution. It is not expected that a wage floor will influence wages at higher levels of the distribution. Table 3.4 shows the results of the model described in equation 2 using the matched data and hourly wage as the dependent variable. Specification 1 indicates that there is a large contemporaneous effect of the minimum wage on workers below the 10th percentile, with a statistically significant elasticity of .25. In contrast, there is no significant contemporaneous effect for workers with higher wages. Living wages have a positive but insignificant contemporaneous effect at the lowest end of the distribution, but a small but significant positive effect at higher percentiles. This

can perhaps be explained by the fact that living wage cities have higher wages overall (see table 3.2). To the extent that living wages are instituted due to higher wages in the cities in question, and the trend in higher wages might continue, the results could be picking up this trend. This might be an indication that the matching strategy employed has not controlled as well as it should for differences in pretreatment wage trends.

The second specification shows the additive effect over time of minimum and living wages<sup>4</sup>. For the lowest percentile, a statistically significant elasticity of 0.23 for minimum wages is seen. Other coefficients are essentially 0, except for a negative elasticity for the 50th to 75th percentile that is statistically significant. For living wages, there are again significant positive effects at each level of the wage distribution.

The final specification for minimum wages indicates an elasticity of 0.14 below the 10th percentile, with a negative effect again at the 50th to 75th percentile. For living wages, the lag indicates an elasticity of about 0.11 for the 10th percentile. A positive effect on wages is again seen at higher levels of the wage distribution, but they have dissipated over time in comparison with the lowest percentile. The results indicate, in general, that living wages have a positive impact on wages for low-wage workers, albeit with a delay.

In contemplating the results, one matter that should be kept in mind is that minimum wages within states are of long standing over the years under analysis, whereas living wages are a recent phenomenon. It makes sense that new living wage laws are likely to show a greater effect after some time has passed and employers have responded to implementation. Another matter to consider is that it would not be unlikely, given the level of the living wage in certain cities, that workers previously in the 0th to 10th percentile would be moved to the 10th to 25th percentile of the wage distribution. This would cause a downward bias on the coefficients at these levels. In the latter case, we can think of the

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<sup>4</sup>I report the sum of the coefficients on the minimum wage and its six-month lag, and the living wage and its six-month lag. The standard error of the sum is reported. The third specification is calculated in a similar fashion.

Table 3.4: Contemporaneous and lagged effects on log wages of workers in various percentile ranges of the wage distribution of SMSAs

| Percentile Range of SMSA's<br>Wage Distribution | <10th            | 10th to 25th      | 25th to 50th      | 50th to 75th      |
|---|------------------|-------------------|-------------------|-------------------|
| Specification 1                                 |                  |                   |                   |                   |
| Minimum wage ( $\beta$ )                        | 0.254<br>(0.055) | 0.045<br>(0.025)  | -0.043<br>(0.023) | 0.000<br>(0.023)  |
| Living wage ( $\gamma$ )                        | 0.028<br>(0.020) | 0.024<br>(0.008)  | 0.023<br>(0.008)  | 0.030<br>(0.008)  |
| Adj. R-squared                                  | 0.123            | 0.487             | 0.475             | 0.490             |
| Specification 2                                 |                  |                   |                   |                   |
| Minimum wage six months ago                     | 0.227<br>(0.062) | -0.022<br>(0.029) | -0.110<br>(0.026) | -0.025<br>(0.026) |
| Living wage six months ago                      | 0.052<br>(.022)  | 0.047<br>(.010)   | 0.052<br>(.009)   | 0.044<br>(.009)   |
| Adj. R-squared                                  | 0.123            | 0.488             | 0.476             | 0.490             |
| Specification 3                                 |                  |                   |                   |                   |
| Minimum wage one year ago                       | 0.138<br>(0.066) | -0.058<br>(0.032) | -0.126<br>(0.029) | -0.041<br>(0.029) |
| Living wage one year ago                        | 0.111<br>(0.025) | 0.066<br>(0.011)  | 0.060<br>(0.010)  | 0.048<br>(0.010)  |
| Adj. R-squared                                  | 0.124            | 0.488             | 0.476             | 0.490             |
| Sample size                                     | 28273            | 34883             | 57648             | 59073             |

The table shows the estimated effects of log minimum wages and living wages on the log wages of workers within the listed percentiles of the wage distribution. Thus the coefficients may be interpreted as elasticities. The sample includes data from each month from 1996 to 2000.

estimate on the one-year lag as a lower bound on the effect of the living wage on wages at the lowest end of the wage distribution.

Finally, a last matter to consider in regards to living wage effects on wages is the unevenness with which wages are implemented across occupations. The matching algorithm used to match workers controlled for occupation; thus, in the analysis, workers who are within a certain occupation are matched between the control and treatment group before parametric analysis occurs. However, workers in different occupations within a city are not compared, and there is always the possibility that wages are reduced for workers who are not covered compared with those who are. The difficulty is in determining who is covered and who is not. It does not seem advisable to use the CPS occupation codes for within-city comparison, as coverage is often based on city contracts that will ensure coverage for some individuals within an occupation and not for others in the same occupation. Additional work is required to determine who is usually covered by these ordinances – until then, the estimates of wage effects will necessarily have to be general, as they are here.

### **3.4.2 Employment effects**

To determine the effect of living wages on employment, equation 2 was again estimated, this time using a dichotomous variable indicating employment status (1 if employed, 0 if unemployed). In the CPS data, unemployment is indicated when an individual reports being laid off or being without a job and looking for employment. To determine the effects of living wages using different ranges of the wage distribution, wages were imputed for the entire sample, including adults not in the workforce. Propensity score matching and calculation of the wage distribution follows as before, with some exceptions. Because those who were unemployed or not in the workforce did not report other characteristics of

employment, union status, government employment, and whether the individual is paid by the hour were necessarily dropped. Following NA, I impute wages using controls for education, age, age-squared, age-cubed, race, sex, marital status, number of family members, number of children below age 18, and fixed year and month of interview.

Table 3.5 reports the results of this analysis<sup>5</sup>. For each specification, the effect of living wages on employment – as well as minimum wages on employment—is not statistically different from 0. In spite of fairly strong evidence that living wage policies promote higher wages among lower-wage workers, there does not appear to be the concurrent decrease in employment predicted by economic theory.

While this finding aligns with similar findings on the minimum wage by some researchers (Card (1992); Card and Krueger (1994)), the result flies in the face of conventional economic theory. There are reasons to doubt the result. Considering the fact that I found an influence of the living wage at all levels of the wage distribution, I might simply be picking up a difference in trends that matching has not adequately accounted for. This is belied, however, by the greater effect seen for those below the 10th percentile in the one-year-lag specification. If the effects on wages are weaker than my initial analysis indicates, then no change in employment is fairly easy to explain away: The uneven application of living wages simply does not bind employers to it, and thus their employment behavior is not affected.

Moreover, in light of uneven application and limited geographic scope, there is always the possibility that lower-wage workers and lower-wage employers both seek opportunities outside of the city when a living wage is enacted; simultaneously, workers and employers for whom the wage does not bind seek opportunities within the city. This would simply shift types of labor and employers to different sectors or different geographic areas, and while wages would look like they increase in the city because of the

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<sup>5</sup>I also ran a parsimonious version using only year, month, and city fixed effects. The results were similar.

Table 3.5: Contemporaneous and lagged effects on employment of workers in various percentile ranges of the wage distribution of SMSAs, imputed wages for population

| Percentile Range of SMSA's  | <10th             | 10th to 25th      | 25th to 50th      | 50th to 75th      |
|-----------------------------|-------------------|-------------------|-------------------|-------------------|
| Wage Distribution           |                   |                   |                   |                   |
| Specification 1             |                   |                   |                   |                   |
| Minimum wage ( $\beta$ )    | 0.033<br>(0.058)  | 0.0713<br>(0.049) | 0.024<br>(0.036)  | -0.010<br>(0.032) |
| Living wage ( $\gamma$ )    | -0.000<br>(0.026) | -0.024<br>(0.021) | 0.033<br>(0.016)  | 0.024<br>(0.014)  |
| Adj. R-squared              | 0.106             | 0.142             | 0.114             | 0.098             |
| Specification 2             |                   |                   |                   |                   |
| Minimum wage six months ago | 0.060<br>(0.068)  | 0.115<br>(0.057)  | 0.061<br>(0.0438) | -0.016<br>(0.038) |
| Living wage six months ago  | -0.005<br>(0.030) | -0.023<br>(0.024) | 0.028<br>(0.018)  | 0.038<br>(0.016)  |
| Adj. R-squared              | 0.106             | 0.142             | 0.114             | 0.098             |
| Specification 3             |                   |                   |                   |                   |
| Minimum wage one year ago   | 0.055<br>(0.077)  | 0.060<br>(0.065)  | 0.085<br>(0.048)  | -0.035<br>(0.043) |
| Living wage one year ago    | -0.006<br>(0.033) | -0.011<br>(0.027) | 0.025<br>(0.020)  | .051<br>(0.018)   |
| Adj. R-squared              | 0.106             | 0.142             | 0.114             | 0.098             |
| Percent employed            | 44.61             | 59.39             | 69.15             | 78.65             |
| Sample size                 | 71716             | 97259             | 162594            | 165517            |

The table shows the estimated effects of living wages and minimum wages on the probability of employment. Wages were imputed for the population between ages 17 and 70, and the distribution of wages calculated for each city-month cell. Elasticities can be calculated by the coefficient divided by the mean percentage reported in the last row of the table.

shift, employment on the individual level would be 0. Future research on employment growth inside and outside of living wage cities would be necessary to answer this question.

While it is a common to use an entire sample to impute wages in this type of analysis, it seems worthwhile to restrict the analysis to workers only, to see if the results are similar. Table 3.6 reports the results when wages are imputed for only those in the workforce. The coefficients of interest again are essentially 0.

Another posited effect of a wage floor is a reduction in hours of the workforce. I returned to the sample of employed workers used to determine wage effect, and calculated the effect of minimum and living wages on reported hours worked per week (changed to log). Results appear in table 3.7. The effect is essentially zero for all segments of the distribution over all specifications, with one exception: there is a slight positive effect of the minimum wage on hours for the 10th to 25th percentile.

## **Conclusion**

This paper refines previous analyses of living wage ordinances and their effects on wages and employment. Following a difference-in-differences strategy similar to that of Neumark and Adams (2003), and using approximately the same data, I match workers in living wage cities to workers in non-living-wage cities based on demographic and employment characteristics that likely affect both the adoption of a living wage ordinance and future wages and employment.

The matching procedure used “preprocesses” the data in order to arrive at treatment and control groups that are closer to the

My results are markedly different from Neumark and Adams. I find a larger positive impact of living wages for those below the 10th percentile in the wage distribution, and no effect on probability of employment for workers in the same distribution of imputed wages. This finding is in closer in accordance with previous work done on individual



Table 3.6: Contemporaneous and lagged effects on employment of workers in various percentile ranges of the wage distribution of SMSAs, imputed wages for all workers

| Percentile Range of SMSA's  | <10th             | 10th to 25th      | 25th to 50th      | 50th to 75th      |
|-----------------------------|-------------------|-------------------|-------------------|-------------------|
| Wage Distribution           |                   |                   |                   |                   |
| Specification 1             |                   |                   |                   |                   |
| Minimum wage ( $\beta$ )    | -0.017<br>(0.060) | 0.000<br>(0.034)  | 0.035<br>(0.020)  | 0.012<br>(0.016)  |
| Living wage ( $\gamma$ )    | 0.036<br>(0.025)  | -0.029<br>(0.014) | -0.001<br>(0.008) | 0.006<br>(0.006)  |
| Adj. R-squared              | 0.044             | 0.027             | 0.015             | 0.010             |
| Specification 2             |                   |                   |                   |                   |
| Minimum wage six months ago | 0.019<br>(0.071)  | -0.002<br>(0.041) | 0.061<br>(0.024)  | 0.022<br>(0.019)  |
| Living wage six months ago  | 0.019<br>(0.030)  | -0.026<br>(0.017) | -0.008<br>(0.010) | 0.003<br>(0.008)  |
| Adj. R-squared              | 0.044             | 0.027             | 0.015             | 0.010             |
| Specification 3             |                   |                   |                   |                   |
| Minimum wage one year ago   | 0.037<br>(0.080)  | 0.024<br>(0.047)  | 0.062<br>(0.028)  | 0.040<br>(0.022)  |
| Living wage one year ago    | 0.010<br>(0.034)  | -0.030<br>(0.019) | -0.010<br>(0.011) | -0.002<br>(0.009) |
| Adj. R-squared              | 0.044             | 0.027             | 0.015             | 0.010             |
| Percent employed            | 86.78             | 93.71             | 95.80             | 96.97             |
| Sample size                 | 36869             | 62009             | 117376            | 134236            |

The table shows the estimated effects of living wages and minimum wages on the probability of employment. Wages were imputed for the population of workers between ages 17 and 70, and the distribution of wages calculated for each city-month cell. Elasticities can be calculated by the coefficient divided by the mean percentage reported in the last row of the table.

Table 3.7: Contemporaneous and lagged effects on hours worked in various percentile ranges of the wage distribution of SMSAs

| Percentile Range of SMSA's<br>Imputed Wage Distribution | <10th             | 10th to 25th      | 25th to 50th          | 50th to 75th      |
|---|-------------------|-------------------|-----------------------|-------------------|
| Specification 1   |                   |                   |                       |                   |
| Minimum wage ( $\beta$ )                                | -0.025<br>(0.101) | 0.209<br>(0.077)  | 0.002<br>(0.048)      | -0.027<br>(0.034) |
| Living wage ( $\gamma$ )                                | 0.015<br>(0.037)  | -0.010<br>(0.028) | 0.0170.008<br>(0.016) |                   |
| Adj. R-squared  | 0.166             | 0.069             | 0.053                 | (0.012)<br>0.060  |
| Specification 2   |                   |                   |                       |                   |
| Minimum wage six months ago                             | -0.057<br>(0.116) | 0.203<br>(0.088)  | 0.000<br>(0.054)      | -0.005<br>(0.040) |
| Living wage six months ago                              | 0.040<br>(0.042)  | -0.011<br>(0.032) | 0.026<br>(0.018)      | 0.004<br>(0.014)  |
| Adj. R-squared  | 0.167             | 0.069             | 0.053                 | 0.060             |
| Specification 3   |                   |                   |                       |                   |
| Minimum wage one year ago                               | -0.081<br>(0.126) | 0.158<br>(0.096)  | -0.009<br>(0.058)     | -0.010<br>(0.042) |
| Living wage one year ago                                | 0.043<br>(0.048)  | 0.003<br>(0.037)  | 0.027<br>(0.020)      | 0.012<br>(0.016)  |
| Adj. R-squared  | 0.167             | 0.069             | 0.053                 | 0.059             |
| Sample size   | 26,293            | 33,204            | 56,084                | 58,034            |

The table shows the estimated effects of living wages and minimum wages on log hours worked. The sample was derived in the same manner as for table 4, with the exception that some observations are missing for usual hours of work per week.

living wage cities. My results are robust to both a parsimonious specification and to imputation of wages based on workers rather than the entire population. I also find no effect on hours worked for the lowest percentile range, and a slight positive elasticity for hours worked for the 10th-25th percentile range, providing evidence of no effect on supply of labor demanded.

For future work, consideration should be made of employment growth in certain occupations in living wage cities, as the unevenness of implementation of ordinances might cause employers and employees to shift sectors or geography, masking a possible employment effect.

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